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TECHNOLOGY QUARTERLY.

VOLUME IV.

BOSTON:
1891.

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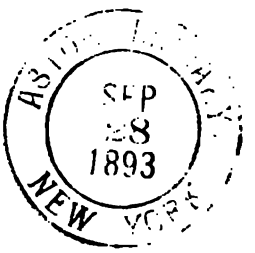

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TECHNOLOGY QUARTERLY.

VOL. IV. APRIL, 1891. No. 1.

THE PROPERTIES, USES, AND PROCESSES OF PRODUCTION OF ALUMINUM.[1]

BY ALFRED E. HUNT, S.B., PRESIDENT OF THE PITTSBURGH REDUCTION COMPANY.

THE metal aluminum has certainly not been "damned by faint praise." On the contrary, from my experience as the president of an aluminum manufacturing company which has been endeavoring for the past two years and over to sell the commercially pure metal in the markets, and to introduce it into the arts and manufactures of the country, I have to state to you as my conviction that one of the two chief difficulties encountered, and which today retards and restricts the wider use of the metal, is the extravagant, erroneous, and in many cases mischievously misleading, statements which have been made as to the properties of the metal, which have received wide circulation, not only through the daily newspapers, but, I am sorry to add, through many technical and scientific journals as well. The other difficulty is the almost equally widespread, and, so far, equally extravagant and misleading, series of claims made by the inventors of processes for the manufacture of aluminum at remarkably low prices.

I shall endeavor in this lecture to correct some of these fallacies as to the properties of the metal, and also, in describing the minerals in which the metal occurs and the methods of reduction which have been practiced, to explain the principal difficulties which have thus far been encountered. I shall also venture a prophecy as to the success of cheaper methods of manufacture of aluminum in the future.

[1] A lecture before the Society of Arts, Massachusetts Institute of Technology, February 12, 1891.

PURE AND IMPURE ALUMINUM.

Some of the erroneous statements regarding aluminum have had their origin in the fact that commercial aluminum is never chemically pure, and that until within the past eight years nearly all the aluminum of which the properties had been described had at least 4% — in many cases 6% or 8% — of impurities. These materially altered the characteristics from those of the purer metal, with not over 2% or at most 3% of impurities, which is ordinarily sold in the markets today.

The impurities almost always found in aluminum are silicon and iron. These two impurities are almost the only ones found in the electrolytically deposited metal, considerable amounts of any others being accidental and due to carelessness in manufacture. In fact, I hope to be able in the near future to say that the impurity consists of the single one, silicon, as we have already made many hundred pounds of aluminum, with only a trace of iron present, and tons of metal with less than $\frac{1}{10}$ of 1% of iron.

The silicon present in aluminum exists in two forms, one seemingly combined with the aluminum, as combined carbon exists in white pig iron, and the other in an allotropic graphitoidal modification. These two forms of silicon seem to exert somewhat different effects by their presence in the aluminum ; the combined form of the element rendering the metal much harder than the graphitoidal variety. The combined silicon ordinarily preponderates, being from 55% to 80% of the total silicon in the average metal of between 98% and 99% purity made by the Pittsburgh Reduction Company.

For many purposes, the purest aluminum cannot be so advantageously used as that containing 3% or even 4% of impurity, as the pure metal is very soft and not so strong as the less pure. It is only where extreme malleability, ductility, sonorousness, or non-corrodibility is required that the purest metal should be chosen. We shall probably find that for most purposes a small percentage of other elements than silicon and iron are advantageously added in producing hardness, rigidity, and strength — constituents that will not detract from the non-corrodibility of the metal as much as do these natural impurities that come from the ore and apparatus — additions that will give the aluminum a better color and greater strength and hardness, with proportionately less sacrifice of malleability, ductility, etc.

COLOR OF ALUMINUM.

Pure aluminum is white, with a decided bluish tint. This becomes much more marked upon exposure, when a thin film of white oxide on its surface prevents further tarnishing from the air, but seems to give, by contrast to the metal as a background, an enhanced bluish tint. I am sorry to hear my good wife report this color as giving a decidedly pewtery appearance to the spoons and forks which we have had in almost constant daily use, for comparison with silver, during the past year and a half. The oxide which gives this decided blue tint may be removed by immersion in a dilute solution of hydrofluoric acid, and then rubbing with chamois skin saturated with petroleum.

No one should expect the color of silver in aluminum — especially of silver of the markets, which is given a decided yellow tinge with copper; for the natural color of aluminum is different, and the addition of copper, although making the metal a yellower white, does not entirely change its natural bluish-white tint. The addition of small percentages of silver, chromium, manganese, wolfram, or titanium, undoubtedly improves the color of aluminum, making it nearer to that of silver.

ACTION OF HEAT ON ALUMINUM.

Aluminum melts at about 1,300° F., but becomes pasty at a temperature of about 1,000° F., and indeed loses its tensile strength and very much of its rigidity at a temperature of 400° to 500° F., although this loss of strength is only while the metal is kept at the high temperature. It serves as an annealing operation, and reduces permanently only the higher tensile strength produced by cold rolling or otherwise cold working the metal. Aluminum does not volatilize at any temperature ordinarily produced by the combustion of carbon, even though the high temperature be kept up for a considerable number of hours. It, however, absorbs a very large amount of occluded gases under such treatment. In the high temperature of the electric furnace, it is claimed that there is considerable volatilization of aluminum, and that small buttons of the metal have been found in and on the linings of the furnace. The Cowles people, however, have never been enabled to collect and commercially produce pure aluminum by their electric furnace, for the reason that the metal rapidly oxidizes into alumina.

It has only been by protecting the reduced metal with an alloy of either iron or copper that they have succeeded at all in retaining the reduced aluminum.

Under heat, the coefficient of linear expansion of $\frac{8}{8}$ in. round aluminum rods of 98.50% purity is .0000206 per degree centigrade between the freezing and boiling points of water; that of iron being .0000122, tin .0000217, copper .000017182. Multiplying this factor by $\frac{5}{9}$ to reduce it to the Fahrenheit scale, we have .0000115, or an expansion of .00115 ft. on 100 ft. of aluminum raised 1° F. from 32°. It is probable that the coefficient of expansion of alloys of aluminum is very close to that of tin.

The specific heat of aluminum is 0.2143, water being taken as 1. The metal follows the general rule of metals, its specific heat being inversely as the atomic weight.

By the method of Weidermann and Franz, taking silver as 100 and copper as 73.6, unannealed aluminum of 98.50% purity has a coefficient of thermal conductivity of 37.96; and the same wire annealed, 38.87. In the same scale, tin has a heat conductivity of only 14.50, iron 11.90, steel 11.60, and lead 8.50. From this it is evident that, next to silver, copper, and gold, soft annealed aluminum is the best conductor of heat.

ELECTRICAL PROPERTIES OF ALUMINUM.

It also stands next to silver, copper, and gold, as a conductor of electricity. One yard of annealed aluminum wire of 98.50% purity, .0325 inch in diameter, at 14° C. has .05484 of an ohm resistance, a yard of pure copper wire of same section having a resistance of .0315 of an ohm. The electrical conductivity of silver being taken at 100, and copper at 90, pure annealed aluminum has an electrical conductivity of about 50. These facts, taken into consideration with the lightness of the metal as compared with silver, gold, and copper, are already leading to a quite extensive use of aluminum in electrical apparatus.

Pure aluminum, and indeed the electrolytically made commercial aluminum, with less than $\frac{1}{5}$ of 1% iron, regularly put upon the market today, exhibits no appreciable polarity. Such metal has many advantages for compass boxes, or for cases for electrical apparatus where non-magnetic properties are desired.

MECHANICAL PROPERTIES OF ALUMINUM.

The fracture of aluminum shows ordinarily hexagonal crystals, although the pure metal is very tough, and in breaking, by bending backward and forward, often appears distinctly fibrous in fracture. The lack of rigidity and hardness which the metal exhibits is a serious obstacle to its adaptability for many purposes, although both stiffness and hardness are very much increased by the addition of a few per cent of impurities or alloy, and are especially improved by cold-hammering, cold-rolling, drop-forging, or the like. For castings for surveying instruments, pure aluminum would be especially adapted were it not for its softness and lack of rigidity. It can be safely stated as a general rule that the purer the aluminum, the softer and less rigid it is.

Pure aluminum is, when properly treated, a very malleable and ductile metal. It can readily be rolled into sheets five ten thousandths of an inch thick, or be beaten into leaf nearly as thin as gold leaf, or be drawn into the finest of wire. Pure aluminum stands third in the order of malleability, being exceeded only by gold and silver; and in the order of ductility, seventh, being exceeded by gold, silver, platinum, iron, softest steel, and copper. Both malleability and ductility are greatly impaired by the presence of the two common impurities, silicon and iron.

Aluminum can be rolled or hammered cold, but the metal is most malleable at between 200° and 300° F., and should be heated to this point for rolling or breaking down from the ingot to the best advantage, and even then considerable power is required, fully as much as for hot-rolling of hard steel of similar section, although not nearly as much draft can be placed upon the rolls as in hot-rolling hard steel. Like silver and gold, aluminum stiffens up remarkably by working it, increasing a great deal its hardness and its tensile strength, and decreasing its malleability and ductility; it requires to be frequently annealed. As compared with cold-rolling soft steel, it has been found practicable to give aluminum about the same reduction in each pass as the steel; the aluminum requiring on the average five annealings where the steel requires but three.

By hardening the metal by rolling, forging, drop-forging, stamping, drawing, etc., it may be turned out very rigid, and will then answer

excellently for purposes where the annealed metal would be entirely too soft, too weak, or lacking in rigidity or elasticity. Especially is this true of aluminum alloyed with a few per cent of titanium, copper, iron, silicon, or the like. The alloys do not show their increased hardness to anything like their maximum extent in castings, and, I am sorry to say, not at all in proportion to the increased brittleness. But when these castings are drop-forged, rolled, hammered, or drawn down, with only sufficiently frequent annealings to prevent the metal from cracking, the increased hardness shows up in a remarkable degree. These properties have already become not only the subject of a considerable number of letters patent, but also of established uses in the arts, and will, in future, add enormously to the applicability of the metal.

ANNEALING ALUMINUM.

To anneal aluminum, a low and even temperature should be maintained in the muffle—just such a temperature as will show an even, red heat in a piece of iron or steel placed in the muffle, when viewed at twilight or on a dark day. The aluminum itself, however, should not appear at all red at this temperature. A ready test of this temperature is that the metal has been heated enough to char the end of a pine stick, which will leave a black mark on the plate as it is drawn across it. When the metal has acquired this temperature, it should be taken from the furnace and allowed to cool gradually. Very thin sections may be annealed by placing into boiling water and either allowing to cool with the water or taking out to cool gradually. Of course it is possible to anneal to any degree by lowering the temperature to which the metal is heated below that specified by means of suitable appliances.

WELDING AND SOLDERING ALUMINUM.

Aluminum can be easily and readily welded by the apparatus of the Thomson Electric Welding Company.

Until very lately the lack of methods for successfully soldering and hardening aluminum were two of the greatest drawbacks to its introduction for many purposes. I am glad to be able to say to you, however, that both of these difficulties, I believe, have lately been successfully evercome. I have already outlined to you the method of alloying pure aluminum with a few per cent of hardening-metal, and cold-rolling

or otherwise working, as being ways out of the difficulty of its softness. The exhibitions of soldered metal will show you that aluminum can now be successfully soldered. These results were obtained by the use of the blow-pipe and with ordinary hard or soft solder, or with pure zinc, or with an alloy of zinc and aluminum, as the soldering metal. The novelty, which has just been covered by letters patent in one successful case, is in the soldering salt, which allows the solder to flow freely on the surfaces to be united.

CASTING ALUMINUM.

Sound castings of aluminum can be readily made in dry sand molds. The aluminum should not be heated very much beyond the melting point; otherwise it seems to absorb gases, which remain in the metal, preventing sound castings. In small quantities the metal can be best melted in plumbago crucibles; but in large quantities it can be more economically melted in a reverberatory furnace with alumina or magnesia brick sides and alumina bottom. The furnace should have a tap-hole for drawing off the liquid metal into carbon-lined ladles. In no case need the metal be covered with a flux to assist in the fusion or to form a covering of slag. In fact, owing to the metal's lightness, the presence of any flux will tend to unsoundness, due to particles of it becoming entangled in the castings, while impurities may perhaps be added to the metal by the action of the flux on the lining of the melting vessel. The molten metal flows readily, and not much larger gates are needed than for brass. The shrinkage of $\frac{15}{64}$ in. per ft., which aluminum has, is considerably more than that of brass, which is about $\frac{3}{32}$ in. per ft. The shrinkage can be reckoned ordinarily as about 2.26% of the length of the mold. Wooden patterns made for brass, to give equivalent castings of aluminum, should be given three or four thick coatings of shellac.

RESISTANCE OF ALUMINUM TO CORROSION.

As to the corrodibility of aluminum, a great deal has been printed that is not correct. In the first place, aluminum is acted upon by the atmosphere, especially by moist atmosphere, and more especially still by moist salt atmosphere. The metal becomes covered by a very thin, almost imponderable, coating of oxide on the surface exposed to the atmosphere, which seems to protect it from further oxidation. This

coating is so thin as often to hardly interfere with any polish, and it does not materially change the weight of the metal. It does, however, increase the bluish tint of the metal, as before explained, and gives it a leadish color. As compared with most metals, pure aluminum, under ordinary circumstances, withstands the action of wind and weather exceedingly well; and many uses to which the metal is now being successfully applied are based upon this fact. The presence of silicon in aluminum materially detracts from its power to withstand corrosion due to atmospheric influences. Metal with 4% or 5% of silicon very soon collects a thick coating of oxide upon it, if severely exposed. The fact that pure aluminum is not acted upon by boiling water or by steam has led to its use as a packing or gasket in steam connections, where lead and similar metals have been rapidly cut out (especially where the water contained notable amounts of sulphur acids), as in parts of steam and water pumps and difficult steam joints. The New York Steam Company have found it a very satisfactory way of packing their leaky steam joints to put in first a layer of aluminum wire, without attempting to calk it much, adding another ring of the softer, but more easily corroded, lead on the outside, which can be calked into place, making the joint steam-tight.

Aluminum containing sodium is rapidly acted upon by hot water, the sodium being eaten out, leaving the aluminum spongy and porous. Aluminum is unaffected by either concentrated sulphuric or nitric acids. Unfortunately, however, these commercial acids almost always contain some little hydrochloric acid, which rapidly corrodes the aluminum, the chloride of aluminum first formed being changed into sulphate or nitrate, the free hydrochloric again acting more violently as it attacks the metal in a nascent state.

Aluminum is not acted upon by carbonic acid, or carbonic oxide gases, nor by sulphuretted hydrogen except at a red heat; but it is a peculiarity of the metal in a melted condition to absorb large quantities of these gases, quite a portion of which is again excluded as the metal cools. But enough is left in the case of the sulphide to emit strong odors of the sulphuretted hydrogen which has been occluded and imprisoned in the metal in cooling. This fact is a strong obstacle to a sulphide method of reduction of the metal, where the sulphuretted hydrogen evolved would strongly impregnate the metal for a long time, even after several meltings.

ALUMINUM FOR CULINARY AND SURGICAL PURPOSES.

Pure aluminum has neither taste nor odor, nor is it corroded by any substances ordinarily used in culinary operations, and it seems, therefore, especially adapted for cooking utensils. For spoons, forks, and large dishes, the metal also is useful, although its color can be improved, and the metal made more rigid, by a slight addition of copper.

Aluminum is less acted upon than is tin or copper or silver by salt water, and even by such solutions in vinegar as the metal is liable to be subjected to in ordinary culinary vessels. The salts of tin or copper thus dissolved are very poisonous; but not only are the aluminum salts that are formed less in amount, but the acetate of aluminum formed resolves itself on boiling into either an insoluble sub-acetate or into pure alumina, neither salt having either taste or injurious toxic action. For these reasons, quite surely aluminum will have a large future for cooking utensils. A cooking skillet of aluminum has been in service for a year in my own household.

Aluminum is found to withstand the action of organic secretions better even than silver, and for many forms of surgical instruments, tracheometer tubes, suture wires, and the like, and for forms for holding false teeth, the metal is already receiving very wide use. Many surgeons are having their instrument cases made of aluminum, it having the advantage that they can safely be washed clean with antiseptic solutions. It may be well for me to say here, however, that perspiration in handling polished aluminum does tarnish the metal, and that aluminum, like other metals, cannot retain a high polish with frequent handling. The natural acid solvent for aluminum is hydrochloric acid. Solutions of the caustic alkalies, chlorine, bromine, iodine, and fluorine rapidly corrode aluminum. Ammonia gas has very little action on the metal, except to turn it a gray color. Strong aqua-ammonia has a slight solvent action upon it.

OTHER PHYSICAL PROPERTIES OF THE METAL.

The specific gravity of aluminum, of course, is one of its most striking properties—a property on which many brilliant "castles in the air" have been built. It runs from 2.56 to 2.70; structural steel is 2.95 times as heavy, copper 3.60 times, ordinary high brass 3.45

times, nickel 3.50 times, silver 4 times, lead 4.80 times, gold 7.70 times, and platinum 8.60 times as heavy. Many uses have been suggested for the metal due to its lightness; one is for the entire framework of air ships. Now as there are many strong woods which have a specific gravity below 1, and which are, therefore, about one third the weight of aluminum, and as woods can be selected that will by no means require three times the section of aluminum to give equal strength for frames, I do not quite understand where the applicability of aluminum for this purpose comes in. Of course, in many cases a much thinner sheet or smaller section of aluminum could be used than of wood; but for purposes where rigidity as well as lightness is required, the parts of air ships could be made much better of wood than of aluminum. I will confess that as aluminum is a favorite metal of mine, I am loth to see it subjected to the rude falls to which, I fear, it will be endangered if attempts are made to build air ships of it.

Pure aluminum is very sonorous, and varying qualities of sound are given by its different alloys. Unfortunately, just the right shape for an aluminum bell has not yet been discovered. The ordinary forms give out anything but an agreeable sound.

THE STRENGTH OF ALUMINUM.

Cast aluminum has about the ultimate strength of cast iron in tension, but under compression it is comparatively weak.

The following table gives the average results of many tests of aluminum, having about the following composition: Aluminum, 97% to 99%; silicon graphitic, 0.10% to 1%; silicon, combined, 0.90% to 2.80%; iron, 0.04% to 0.20%.

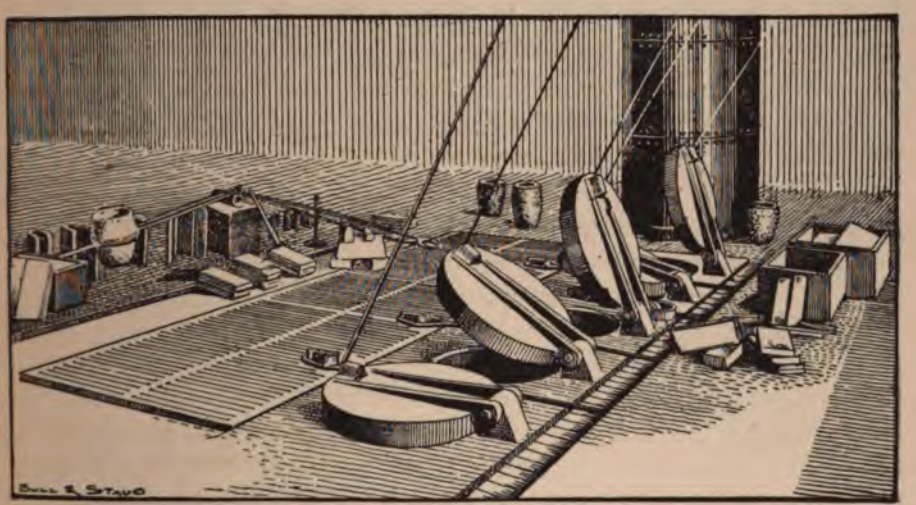
Tension tests.	Castings.	Sheet.	Wire.	Bars.
Elastic limit per sq. in., lbs. . .	6,500	12,000	16,000 to 30,000	14,000
Ultimate strength per sq. in., lbs.	15,000	24,000	30,000 to 60,000	28,000
Reduction of area	15%	35%	60%	40%

Compression tests (in cyclinders with length twice the diameter):

Elastic limit per sq. in.	3,500 lbs.
Ultimate strength per sq. in.	13,000 lbs.

The modulus of elasticity of cast aluminum is about eleven million; cold drawn aluminum wire, about nineteen million; aluminum sheets, about thirteen million.

Under transverse test pure aluminum is not a very rigid metal. An inch square bar of good cast iron, supported on knife edges 4 ft. 6 ins. apart and loaded in center, will readily stand 500 lbs. without a deflection of over 2 ins. A similar bar of aluminum would deflect over 2 ins. with a weight of 250 lbs., although the aluminum bar would bend nearly double before breaking, while the cast-iron bar will ordinarily break before the deflection has gone very much beyond 2 ins. Aluminum to withstand strains, and especially to have good elasticity, should be alloyed with a few per cent of impurities and cold-rolled, or otherwise worked cold. In this way it can be made to be nearly as strong and elastic as mild steel, section for section, and weight for weight will be much more rigid.



MELTING HOLES — PITTSBURGH REDUCTION COMPANY'S PROCESS.

ALLOYS OF ALUMINUM AND COPPER.

Aluminum and copper form two series of valuable alloys; the aluminum bronzes, ranging from 2% to 12% aluminum with copper, and the copper hardened aluminum series, with from 2% to perhaps 15% of copper with the aluminum. The aluminum bronze series are already beginning to assume an important position in the arts, and I believe will very largely replace brass and other bronzes, and for some purposes

take the place of steel. By adding 8% to 12% of aluminum to copper, we obtain one of the densest, finest grained, and strongest metals known — a metal having a remarkably good ductility as compared with its tensile strength. A 10% bronze can readily and uniformly be made in forged bars, with 100,000 lbs. per square inch tensile strength, with 60,000 lbs. elastic limit per square inch, and with at least 10% elongation in 8 ins., and aluminum bronzes can be made to meet a specification of even 130,000 lbs. per square inch and 5% elongation in 8 ins. Such bronzes have a specific gravity of about 7.50, and are of a light yellow color. The 5% to 7% aluminum bronzes have from 8.30 to 8 specific gravity, and a handsome yellow color. They readily give 70,000 to 80,000 lbs. per square inch tensile strength, with over 30% elongation in 8 ins. and with an elastic limit of over 40,000 lbs. per square inch. It will probably be alloys of the latter character that will be most used, especially for marine work. The fact that 5% to 7% bronzes can be rolled or hammered at a red heat, proper precautions which can readily be secured being taken, will add greatly to their use. Metal of this character can be worked in almost every way that steel can, having for its advantage greater combined strength and ductility, and much greater power to withstand corrosion. By far the best results have been obtained by using the purest aluminum and purest copper. The presence of silicon in aluminum alloys makes a harder bronze, but one of much less comparative ductility and malleability. The presence of iron weakens and very seriously interferes with the value of bronze. Tin also is disadvantageous. The presence of zinc in the aluminum bronze is not so deleterious; in fact, it makes the best aluminum brasses much better than those having tin in them.

The Cowles and Heroult people, who by their methods manufacture aluminum bronze by direct reduction, have claimed that the bronze directly reduced was more homogeneous and better than that made by melting together copper and pure aluminum. Careful experiments made in this matter, however, have failed to show this claim to be true. From my experience, I entirely agree with Professor Richards, who, in his excellent work, says: "Two specimens containing nothing but aluminum and copper in like proportions will be identical, no matter how they are produced." As it is far easier, and at present cheaper, to produce a bronze with less iron and silicon by using pure aluminum of less than 2% of these impurities, which with

pure copper will give less than $\frac{1}{5}$ of 1% impurity in the bronze, the advantage is certainly on the side of the use of pure aluminum, melting it with pure copper, so far as quality is concerned.

The aluminum in bronzes lowers the melting point of the copper at least 100° or 200°. The melting point of 10% aluminum bronze is somewhere in the neighborhood of 1,700° F.

WORKING ALUMINUM BRONZE.

Aluminum bronze is among the hardest of the bronzes, and hardens upon cold-working considerably. This hardness, however, can readily be lowered by annealing at a red heat and plunging into cold water. Aluminum bronze can readily be worked in a lathe, and the chips cut clean, smooth, and long, and do not clog the tool. Aluminum bronze is a remarkably rigid metal under transverse strain, being much more rigid than ordinary brass, or even gun bronze. Under compression its strength, although rather low in elastic limit compared to its ultimate compressive strength, is still much higher than that of any of the other bronzes, and there is a long period of gradual compression before finally giving way, making it peculiarly a safe metal under compression.

Aluminum bronze has excellent anti-friction qualities, owing to its fine grain texture and peculiar smooth and unctious though hard surface, which resists abrasion remarkably. Attention has already been called to the anti-corrosive qualities of aluminum bronze; and as its electrical conductivity is better than that of brass, it is peculiarly well adapted for commutator bars in dynamos. It can be brazed and soldered nearly as well as brass.

CASTING ALUMINUM BRONZE.

Sound, clean castings of aluminum bronze can be safely and regularly made, either in sand molds or against chills, if the proper precautions are taken to avoid: first, oxidation; second, contamination from scum or a cinder composed of oxide of aluminum with a little copper in it; third, depressions, cracks, or strains due to shrinkage; fourth,the shutting in of gas into the castings. The first trouble, oxidation, is due to heating the metal too hot in the plumbago crucible. The copper should not be heated much above its melting point before the aluminum is added, which at first absorbs a great deal of heat and

rapidly thickens the metal, the aluminum being rapidly immersed in the copper to prevent its oxidation. Great care should then be taken in again bringing the metal up to the liquid condition, and the metal should be frequently stirred; for it suddenly heats up by the reaction of the alloying of the two metals, and, unless precautions are taken, will soon bring the temperature up to a white heat, where oxidation will seriously injure the bronze. The metal should have a cover of powdered charcoal to prevent oxidation, but with no other flux over its surface.

The second trouble, contamination from the scum, can be avoided by pouring from the melting furnace or pot into a hot ladle or pouring basin large enough to hold all the metal necessary to fill the mold and to allow the metal to pour out from the bottom of such a receptacle after giving sufficient time to allow the scum to come to the surface. A skim-gate should also be provided for each mold.

The third difficulty is caused by a peculiar red-shortness just after solidification, and also a contraction at solidification which causes the metal to either tear apart or yield and cause depressions in the casting at more favorable spots, if any great resistance is experienced by the metal in contraction. This difficulty is overcome by giving bountiful allowance for the contraction, which can be done in several ways, each best adapted for varying conditions. The cores should be made of a yielding character, using resin or other suitable substance with coarse sand, that will yield to slight pressure. Unyielding iron core rods should be dispensed with as far as possible. "Green" sand cores will give good results in some cases. Other expedients to gain the same ends will be easily suggested by skilled molders.

The troubles due to shrinkage are avoided by having the "risers" or "feeding heads," with flaring openings, large in section, often larger than the castings they are intended to feed, and if necessary refilling the feeding heads, in some cases several times over. In this way the casting will solidify first, drawing metal to supply its shrinkage from the still fluid riser having a level higher than the casting itself, so that the metal will flow down into it. The gates to the mold should be sufficient in number and so arranged that they can be filled with the metal as cold as it will pour and give full castings. The considerable amount of scrap occasioned by the large sectioned risers can be remelted without loss of aluminum, and, indeed, is improved in quality by the process of remelting.

To prevent the shutting in of gas into the castings, dry sand should be used, and the molds should be well vented by the ordinary precautions taken by founders for this purpose. Although it requires skill and experience to successfully carry out the conditions outlined above, sound, clean castings, even of the most difficult sections of aluminum bronze, can be made regularly, and with no more wasters than with ordinary brass, and without the difficulty of a copious liberation of gas at the moment of solidification, as is the case with steel. In fact, aluminum bronze, with its low pouring temperature, is especially applicable for massive and heavy castings.

ALLOYS WITH SMALL PERCENTAGES OF COPPER.

The alloys of aluminum with copper in proportion of from 2% to 15% have already been incidentally referred to. Copper, I believe, is one of the best hardeners of aluminum. The maximum proportion that can be added before the metal becomes too brittle to work depends largely upon the purity of the aluminum. If it contains several per cent of silicon it becomes very brittle with 5% of copper; but with pure aluminum it is still forgeable, with difficulty, with 15% of copper. Even very small percentages of copper added to pure aluminum decrease the blue color of the metal and give a more pleasing yellow-white color to it.

Proportions of a few per cent of copper added to pure aluminum decrease the shrinkage of the metal and give alloys that are especially adapted for art castings. The remainder of the range, from 15% copper up to over 85%, give crystalline and brittle alloys of no use in the arts, and of a grayish-white color, up to 80%, where the distinctly yellow color of the copper begins to show itself.

There is said to be an alloy of 50% copper and 50% aluminum, which is malleable. I have, however, been unable to discover any such alloy in experiments which I have made in this direction, all of the alloys being very crystalline and brittle.

ALUMINUM WITH IRON AND STEEL.

Aluminum combines with iron in all proportions. None of the alloys, however, have proved of value, except those of small percentages of aluminum with steel, cast iron, and wrought iron. So far as experiments have yet gone, other elements can be better em-

ployed to harden aluminum than iron, and its presence is regarded as entirely a deleterious impurity, to be avoided if possible.

The addition of from ⅓ lb. to 2 lbs. of aluminum to a ton of steel gives the advantage of quieting the metal in the mold and producing ingots with much sounder tops, so that the scrap due to crop ends is materially lessened, a saving which much more than pays for the cost of the aluminum added. The amount to be added varies with the character and condition of the steel. With well-melted steel, low in carbon, the proportion of from ⅓ to ½ lb. to the ton seems to give the best results; any larger proportion making the metal pipe and causing excess of crop-end scrap. If the steel be "wild" in the ladle, full of occluded gases, too hot, or oxidized, a larger proportion of aluminum can be advantageously added. Mr. R. A. Hadfield says, I think correctly, that the influence of aluminum appears to be like that of silicon, though acting more powerfully. The same writer, together with Professor H. M. Howe and Mr. Osmund, claims that an addition of aluminum does not lower the melting point of steel. In this my own experience agrees; nor do I believe that aluminum increases to any great extent the fluidity of the steel. So far as my experience goes, steel, with an addition of $\frac{1}{10}\%$ of aluminum, seems to solidify in the molds fully as quickly as steel without the addition of the aluminum. Aluminum seems to take the oxygen out of steel very much in the same way that manganese does, but I am not aware of any advantages gained by using the more expensive aluminum for this purpose.

The addition of aluminum in quantities of from 2 to 3 lbs. per ton is of advantage where the steel is to be cast in heavy ingots which will receive only scant work. Here it seems to increase the ductility as measured by the elongation and reduction of area of tensile test specimens, without materially altering the ultimate strength.

The question as to whether additions of aluminum prevent segregation of impurities in large castings of steel, is still an unsettled one; but I may say that it is now being carefully investigated.

In steel castings the benefit from the use of a small percentage of aluminum, ordinarily in the proportion of from 2 to 3 lbs. per ton, has become widely recognized, and it is being generally used.

The additions of aluminum are almost always made by throwing the metal, in pieces weighing a few ounces each, into the ladle, as the

steel is pouring into it. In cast iron, from 2 to 5 lbs. of aluminum per ton is put into the metal as it is being poured from the cupola or melting furnace. To soft gray No. 1 foundry iron it is doubtful if the metal does much good, except, perhaps, in the way of keeping the metal melted for a longer time; but where difficult castings are to be made, where much loss is occasioned by defective castings, or where the iron will not flow well, or give sound and strong castings, the aluminum certainly in many cases allows of better work being done and stronger and sounder castings being made, having a closer grain, and hence much easier tooled. The tendency of aluminum is to change combined carbon to graphitic, and it certainly lessens the tendency of the metal to chill. Aluminum in proportions of 2% and over materially decreases the shrinkage of cast iron.

The effect of aluminum in wrought iron is not very marked in the ordinary puddling process. It seems to add somewhat to the strength of the iron, but the amount is not of sufficient value to induce the general use of aluminum for this purpose.

The peculiar property of aluminum in reducing the long range of temperature between that at which wrought iron first softens and that at which it first breaks down and becomes fluid, is taken advantage of in the well-known Mitis process. It is for this that aluminum is most used in wrought iron at present.

ALUMINUM AND OTHER METALS.

With the exception of lead, antimony, and mercury, aluminum unites readily with all metals; and many useful alloys of aluminum with other metals have been discovered within the last few years. I venture to prophesy the finding of many more in the near future, as more systematic attempts are made to study the use of aluminum with other metals and metalloids. Of many of these useful improvements that are now subjects of letters patents, for obvious reasons, I will say nothing. The useful alloys of aluminum so far discovered are all in two groups, the one of aluminum with not over 15% of other metals, the other of metals containing not over 15% of aluminum; in the one case, the other metals imparting hardness and other useful qualities to the aluminum, and in the other the aluminum giving useful qualities to the other metals.

The alloy of a few per cent of silver to aluminum to harden, whiten, and strengthen the metal, gives a metal especially adaptable for many fine instruments, tools, and electrical apparatus, where the work upon the tool and its conveniences are of more consequence than the increased price due to the addition of the silver. The silver lowers the melting point of aluminum and gives a metal susceptible of taking a fine polish and making fine castings.

More or less useful alloys have been made of aluminum with zinc, bismuth, nickel, cadmium, magnesium, titanium, chromium, manganese, and tin — these alloys all being harder than pure aluminum; but it is by combinations of these metals, with additions perhaps of copper, lead, and antimony, that alloys of most useful value have so far been discovered. Some are with additions of only 1% to 2% of aluminum.

The addition of from 5% to 15% aluminum to type metal composed of 20% antimony and 80% lead makes a metal giving sharper castings and a much more durable type.

To ordinary brass, the addition of aluminum gives superior strength and better anti-corrosive qualities. Some very marked and valuable qualities have been discovered in the use of aluminum with zinc for various purposes.

The modifications of pewter, Britannia, white metal, Delta metal, and the like, with additions of aluminum, have shown very useful qualities, and will add very considerably to the demand for aluminum in the near future.

CLAY AS A SOURCE OF ALUMINUM.

Evidently contrary to the understanding of the case by many people who have written us with brilliant claims of rich mines, aluminum has never been found in nature in the metallic state; and yet the general statement made by Professor Richards is true, that "there is no other metal on the earth so widely scattered, and that occurs in such abundance." This statement, however, has been interpreted by the public to the effect that therefore all these varied minerals containing aluminum are equally applicable as ores from which to extract the metal; especially have they considered that clays, silicates of aluminum, are the specially chosen ores for the production of aluminum. A consideration of the comparative constituents of clay with those of the minerals given below shows the disadvantage of the rich-

est of clays, as aluminum ores, as compared with the richer and purer oxides, fluorides, and sulphates, which minerals are now used as ores of aluminum.

Bauxite ($Al_2H_6O_6$), soft and granular; with 50% to 70% oxide of aluminum and with only a few per cent of accidental impurities besides the water of hydration.

Corundum (Al_2O_3), very hard and crystalline, specific gravity 3.909.— With 93% alumina and ordinarily very free from impurities, but so hard and crystalline, and withal so valuable for other purposes, as not to be at present used as an aluminum ore.

Diaspore ($Al_2O_3H_2O$), hard and crystalline; specific gravity, 3.4.— With 65% to 85% alumina and ordinarily very pure.

Cryolite ($Al_2Fl_6 6NaFl$), specific gravity, 2.9.— With 40% aluminum fluoride and 60% sodium fluoride.

Aluminite ($Al_2SO_6 9H_2O$), specific gravity, 1.66.— Containing some 30% of alumina in a condition to be cheaply purified by solution, filtration, and roasting.

Gibbsite ($Al_2O_3 3H_2O$), stalactitic · specific gravity, 2.4.— Containing about 65% alumina.

In comparison to these, the clays vary from tri-basic silicates of alumina to pentacid silicates, minerals with at best 65% alumina and 35% silica; while the more common clays contain from 50% to 70% silica and only from 50% to 35% alumina. Pure kaolin contains 39% alumina, carrying about 20% aluminum, with 40% silica, and 14% water. It has a formula of $Al_2Si_2O_7 2H_2O$. Now silica is much easier reduced than alumina, and by all the methods of reduction this large percentage of silica must be separated from the alumina before the reduction is commenced, else a large percentage of energy used in reduction will be expended in reducing a troublesome impurity, instead of in reducing the alumina to aluminum. The true value of clay as an ore of the metal is evident, therefore, in comparison with the richer and purer ores that have only accidental amounts of iron and silicon, up to perhaps 10%, to be removed before being submitted to the reducing operation.

There seems to be one fact favorably prominent, however, in the prospect of fat clays like the kaolins being used as aluminum ores — that is, that concentrated cold sulphuric acid dissolves the alumina out of most clays, leaving the silica insoluble. Although this fact has not yet, to my knowledge, been made use of on a commercial scale, still in the future, when there is a large demand, this method may make clays available as ores of aluminum.

PREPARING ALUMINUM ORE FOR REDUCTION.

Bauxite is the mineral most used at present for the manufacture of alumina, and it probably will continue to be the most economical ore, for the reason that it is soft, easily ground and easily decomposed to separate the impurities of silica and iron. The mineral occurs in great abundance in the South, in Tennessee, Virginia, North and South Carolina, Georgia, Alabama, and also Arkansas. Very pure bauxites can be laid down in quantities in Pittsburgh at a cost of between $9 and $10 per ton, and with only the cost of mining and transportation can be put on cars nearest the mines in many cases at a cost of less than $2.50 per ton.

The general method of purification of the ore is to calcine the bauxite with just sufficient sodium carbonate to form sodium aluminate ($Al_2O_3 3Na_2O$). The temperature must be kept below the melting point (the aluminate being formed at a temperature lower than the melting point of the mixture), else silicate of aluminum is formed. The mass is well stirred to insure the complete displacing of all the carbonic acid and the combination of the sodium carbonate with the alumina. A ready test of the completion of the reaction is that portions taken from the mass no longer effervesce and evolve carbonic acid upon treatment with acid. The incinerated mass is taken out of the furnace, ground and lixiviated with hot water, which takes the sodium aluminate into solution, leaving the silica and iron insoluble. The clear supernatant sodium aluminate solution is siphoned off, and the alumina is precipitated with carbonic acid gas. This forms sodium carbonate, which remains in solution, while the alumina settles out to the bottom of the tank. This is afterwards washed with hot water and dried.

The same general method of treatment is applicable to the aluminum oxide minerals for their preparation; the difficulties to overcome being in grinding the minerals, because of their hardness and the formation of the sodium aluminate at a low enough heat to prevent the sodium silicate forming. It may be that these minerals had best be taken into solution as sulphates, and thereafter treated as the native sulphates by a red heat to drive off the sulphuric acid, leaving pure alumina as a residue. With proper apparatus to condense and save the sulphuric acid, such processes may not be excessively expensive. However, neither this method of manufacture of the

sulphates from clays or from native oxides, nor the method of manufacture of alumina from the sulphate by roasting, have so far found favor commercially in comparison with the soda carbonate fusion method for the preparation of the oxide. The sulphate of aluminum melted with sulphur gives off sulphuric acid gas and transforms the alumina into sulphide. This reaction has been made available in one of the methods of manufacture of aluminum from sulphide by the aid of electricity; the sulphide being afterwards decomposed by electrolysis.

COMPOUNDS OTHER THAN ALUMINA FROM WHICH ALUMINUM HAS BEEN REDUCED.

Aluminate of soda is made from cryolite by the incineration of finely ground cryolite with an intimate mixture of chalk or finely ground carbonate of lime, with enough ground coke to keep the mixture porous. Soluble aluminate of soda is formed; insoluble fluoride of lime remains as a waste product, together with carbonic acid which is evolved. The precipitation of the alumina from the aluminate of soda is carried on by the same method as previously described.

Aluminum sulphide, a yellow, infusible salt, containing 36% aluminum, is produced from alumina by the reaction of carbon bi-sulphide at a high heat in the presence of carbon. It is also prepared, as explained, by the fusion of aluminum sulphate in the presence of sulphur. It is a rather unstable compound, being decomposed by standing in moist air into alumina, with the evolution of sulphuretted hydrogen.

The chloride of aluminum was for a long time the chief salt from which the metal was produced, for the reason that it had been found that metallic sodium reduced the metal from this salt the easiest. For a long time the only practical way of cheapening the manufacture of aluminum by this method was to cheapen the manufacture of sodium and reduce the expense and improve the quality of the aluminum chloride. This proved a difficult task.

The salt varies from pure white, when pure, through the yellows to a brick red when largely contaminated with ferric oxide. It is produced by the action of chlorine gas upon an incinerated mixture of alumina with tar or some oil that will, on burning out during the incineration, leave its coke in intimate mixture with the alumina.

The double chloride of aluminum with sodium is produced if a supply of salt is added to the mixture and it is treated with chlorine gas. The expense and difficulties connected with the manufacture have proven very large, and the chlorine produced has to be very carefully made to keep it free from iron. The difficulties connected with the generation and use of chlorine gas on a commercial scale are found to be very great and the deterioration of the apparatus to be very serious.

The native fluoride of aluminum and sodium, the mineral cryolite, has been used in several processes as the ore for the production of aluminum. It, however, contains too much silicon and oxide of iron to make pure metal; for, as before said, in all the methods so far devised, or which seem likely to be devised in future, the agents that serve to reduce the aluminum to a metallic state will surely reduce all the silicon and iron from their oxides that may be present at the same time or rather previous to the reduction of the aluminum. Pure aluminum fluoride (Al_2F_6) is produced by the action of sulphate of aluminum on cryolite, sulphate of soda being the by-product, according to M. Grabau. Aluminum fluoride can also be produced by the action of hydrofluoric acid on alumina. It is not such an expensive or difficult salt to prepare as the chloride, and is the compound from which aluminum is produced in many of the later and more successful processes. Metallic sodium reduces the fluoride of aluminum with the same facility with which it acts upon the chlorine compounds.

METHODS OF ALUMINUM MANUFACTURE.

Aluminum cannot be reduced from its oxide by the aid of carbon as a reducing agent, because the temperature to which the intimate mixture of the solid carbon and the alumina has to be raised can only be attained by the highest heat of an open-hearth furnace or in the electric furnace — a temperature at which the aluminum reduced cannot itself be accumulated into a molten liquid mass, and can only be retained by cloaking it with a more stable metal like iron or copper. It is for this reason that the Cowles Electric Smelting & Aluminum Company are not able in their electric furnace to manufacture commercially pure aluminum, nor the Heroult people to manufacture the metal from their process of reduction, where they electrolyze pure molten alumina, which requires a high electrically produced heat to

melt the oxide. None of the other salts are susceptible of being reduced by carbon at much lower temperature than the oxide, so far as yet discovered.

Debarred from using carbon as the reducing agent under the ordinary conditions which make it the practicable and economical reagent in most metallurgical operations, the advantages of other stronger reducing agents have been carefully tried. So far only one has proven commercially available, although there are many other agents capable of reducing the metal from its oxide. Metallic sodium reduces the metal from its chloride or from its fluoride salts with ease. Methods based upon the use of sodium as the reducing agent have until lately given not only the purest but the cheapest aluminum. These methods, however, of late, have been succeeded by the cheaper and more direct processes of electrolysis of some of the aluminum salts or of the pure oxide.

THE HISTORY OF ALUMINUM PRODUCTION.

So far as history informs us, Wöhler in 1827 first reduced metallic aluminum by the aid of metallic potassium as the reducing agent from aluminum chloride. The metal was a dry powder in a finely comminuted state; it was very impure, and was only a metallic curiosity. Deville, twenty-seven years later, in 1854, was the first to produce the metal in any quantity, or with any degree of purity.

It is curious to note that the first pure aluminum made was by electrolysis; both Bunsen and Deville reduced the double chloride of aluminum and sodium by electricity generated by galvanic batteries. But as at that time the dynamo was still a machine of the future, electrolysis was soon abandoned for the sodium process by the successful concerns in the manufacture of the metal, although continued experimental efforts were made with electricity from that time to this. The first aluminum made in 1855 was valued at $90 per lb. In 1857, with the development of the sodium method of reduction, the price was lowered to from $28 to $32 per lb. In 1860, the price was again lowered to about $17 per lb., and from 1862 to 1887 the price ranged from $12 per lb. upward, according to purity. In 1887 the price was reduced to one pound sterling per pound avoirdupois; and it was at this price that the first metal made was sold by the new concerns built during that year. In 1889 the Pittsburgh

Reduction Company reduced the price to $2 per lb. As the English works of the Pittsburgh Reduction Company commenced in 1890 to sell the metal in England at five shillings ($1.21) per pound, I presume that future writers will record a further drop in the price of the metal in 1891.

About 1857 the famous works at Salindres were established, which were under the proprietorship of Pechiney & Company for many years, and, until within the past three years, enjoyed not only the reputation of making the best, but the largest, amount of aluminum produced in the world. The care and skill shown, and the ingenious devices and precautions taken by the firm to prevent impurities in the metal by the cumbersome and expensive sodium process in which there were so many opportunities for the addition of impurities, were worthy of the highest praise. In 1860 Sir I. Lowthian Bell, with a brother, started works in Newcastle-on-Tyne, which were abandoned in 1874. They manufactured by the sodium process. From 1874 until 1882 the French company at Salindres was the only concern making pure aluminum.

In 1882 Webster organized the "Aluminum Crown Metal Company" at Hollywood, near Birmingham, England, and by cheapening the production of aluminum chloride soon developed a successful concern. This was further strengthened by the improvement of Mr. H. Y. Castner, an American chemist, who, in 1886, patented improvements for producing a more intimate mixture of the carbon with the caustic soda in a state of fusion by means of a carbide of iron, in this way cheapening by more than one half the cost of manufacture of metallic sodium, which had been $1 per lb. This concern was organized under the name of the Aluminum Company, Limited, and put up a large and expensive plant at Oldbury, near Birmingham, England. These works were started at the end of June, 1888, and they have been manufacturing up to within the past six or nine months. In common with the other manufacturers by the sodium process, they have been working at great disadvantage since the advent of the more successful electrolytic processes.

Early in 1888 the Alliance Aluminum Company started works at Wallsend-on-Tyne, England, using a process which was an innovation upon the Deville sodium process by using the fluoride or the double fluoride of aluminum and sodium cryolite as the compound to be

reduced instead of the chloride or the double chloride of the metal. Professor Nette, the managing director of the concern, also had a process for producing metallic sodium cheaply by allowing fused caustic soda to trickle over incandescent charcoal in a vertical retort. Some very excellent aluminum was produced at this works; they became involved in a lawsuit, however, with the Aluminum Company, Limited, and the newer processes coming up caused them also to close their works, which have now been shut down for over a year.

ALUMINUM PRODUCTION BY ELECTRICITY.

In spite of the broad claims of the Cowles Brothers to the use of electricity for metallurgical purposes, the first actual reduction of aluminum in a metallic state on a practical scale by Deville in 1854 was, as we have seen, by the aid of electricity, electrically decomposing the chloride. Even then the idea was old, for Sir Humphry Davy in 1810 publicly described the successful experiment made in 1807, in which he connected the negative pole of a battery of 1,000 double plates with an iron wire which he heated to a white heat and then fused in contact with moistened alumina, the operation being performed in an atmosphere of hydrogen. The iron upon analysis was found to be alloyed with aluminum.

Bunsen in Germany and Deville in France, in 1854, each electrolyzed the double chloride of aluminum and sodium. Le Chatelier obtained English patent No. 1,214 in 1861, and Monckton, in 1862, English patent No. 264 for the reduction of aluminum by aid of electricity. In fact, Richards says that the Monckton patent "proposes to pass an electric current through a reduction chamber, and in this way to raise the temperature to such a point that alumina will be reduced by the carbon present," showing that not only was the use of electricity for reducing aluminum old, but that the idea of the electric furnace was twenty years old at the date of the first of the Cowles patents in 1885.

Richards, in his work on aluminum, mentions and gives the published authority in each case of the processes for reducing aluminum with the aid of electricity; of Gaudin, 1869; Kagensbusch, 1872; Berthaut, 1879; and Gratzel, 1883.

The newer pure aluminum processes using electricity of Hall, Heroult, and the Bernard Brothers, with the help of Minet, together

with the alloy processes of the Cowles Brothers and of Heroult, are the only ones that are now being worked upon a commercial scale, so far as I know, though perhaps the meritorious inventions of Mr. Grabau may have been reduced to practical working of late. Of this I am uncertain, or of what is being done by Herr Saarburger at Hemelingen, near Bremen. I only know that we no longer hear of the Aluminum & Magnesium Fabrik Company's aluminum in the competition of the markets, nor of any metal made by the sodium process.

Besides these, there have been a host of other electrical attempts and experiments for the manufacture during the past two years, since the success of some of the other companies using electricity became known.

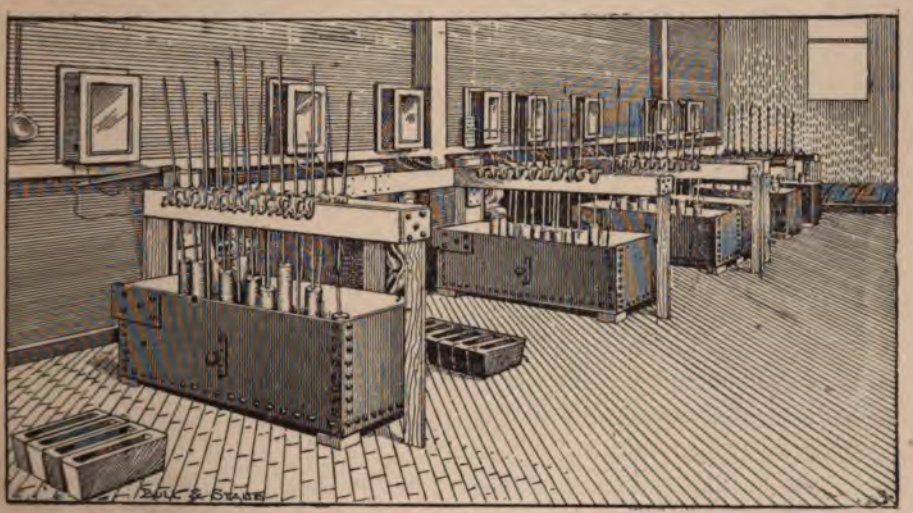
THE HALL PROCESS OF ALUMINUM PRODUCTION AND OTHER ELECTRIC PROCESSES.

The Hall process of manufacturing aluminum is owned by the Pittsburgh Reduction Company, who have, in addition to 390 electrical horse power at work in Pittsburgh, another plant of about equal size at Patricroft, Lancashire, England, the Pittsburgh works commencing to manufacture in November, 1888, and the English works in July, 1890.

The Hall process consists in electrolyzing alumina dissolved in a fused mixture of fluorides of aluminum and sodium or of fluorides of aluminum and potassium, or, in fact, Mr. Hall has covered in his letters patent No. 400,667 a fused bath in which the alumina is dissolved in the fluoride of aluminum, together with the fluoride of *any* metal more electropositive than aluminum.

As the Pittsburgh Reduction Company uses the process, it places the mixture of fluoride salts in a row of carbon-lined iron tanks placed in series. The pots, together with their carbon linings and the reduced metal in the bottom of the pots, become the negative electrodes or cathodes. The positive electrodes or anodes are a series of 3-inch diameter carbon cylinders, attached by ⅜-inch copper rods to the copper conductors by the aid of suitable binding screw clamps. The current of 5,000 amperes and fifty volts in one series, and of 2,000 amperes and 20 volts in the other series, is turned on, and the mixture is melted by the heat caused by the resistance offered to the current by the fluoride mixture. In less than

two hours' time the mixture becomes fluid, and alumina is added. The electrolyte then becomes a much better conductor, "the resistance of the pot" goes down to a normal one of about 8 volts, and the operation of electrolysis commences. The alumina in solution, or, as some claim, the fluoride of aluminum, is decomposed; the metal, being heavier than the electrolyte, sinks to the bottom of the pot, and the oxygen goes to the positive carbon electrode, uniting with a portion of the carbon and escaping as carbonic acid gas; or, as is maintained by some, the aluminum of the fluoride of aluminum is deposited, and



REDUCING POTS — PITTSBURGH REDUCTION COMPANY'S PROCESS.

the fluorine attacks the dissolved oxide, reforming fluoride of aluminum, and thus maintaining the integrity of the original electrolyte bath, the oxygen going off, as in the other case, at the positive electrode.

While the actual fact as to which reaction takes place is not essential to the validity of the Hall patents, and the Pittsburgh Reduction Company makes no claims regarding the matter, it is our belief that the compound electrolyzed is alumina. I may say, in this connection, that this is the view of Professor J. W. Langley, Professor Richards, and of all the other metallurgists who have had experience with and have given the actual metallurgical operations careful study.

Among the reasons for our belief are the heat of formation of alumina, 391,600 calories; the fact that it requires only 2.8 volts to decompose it; the probable fact that the heat of formation of the aluminum fluoride is very considerably greater; the fact already established, that it requires a very much higher voltage than 2.8 to decompose the electrolyte that we use; and the fact, already mentioned, that when the bath gets out of alumina the resistance rises considerably (ordinarily about four volts), and that the heat rises rapidly in the pot until the electrolyte itself begins to decompose, and stifling white fumes of hydrofluoric acid begin to be given off.

Again, fluorine displaces oxygen in alumina; hence we reason that aluminum fluoride is a stronger compound than alumina. Sodium reduces aluminum fluoride — the basis of the Grabau process. Ergo, sodium fluoride is a stronger compound than aluminum fluoride. Hence, in a bath containing sodium fluoride, aluminum fluoride, and alumina, in nearly equal proportions, the order in which they will be decomposed by an electric current of graduated intensity, or in conditions similar to the Hall bath, will be alumina, aluminum fluoride, and sodium fluoride. Again, the electro-motive force required to decompose compounds is in proportion to the heat evolved in the formation of electro-chemical equivalent weights of the compounds; and sodium fluoride is thermally much greater than $\frac{1}{6}$ of a molecule of aluminum fluoride, or of a molecule of alumina.

According to my understanding of the case, the difference between the Hall process and the Heroult alloy process is this: Hall electrolyzes alumina made fluid at a lower temperature by dissolving it mechanically in a fused fluoride solvent, lighter than aluminum, which will not corrode the metal, and above all will not be itself decomposed by the electric current (or at least will not stay decomposed), and which is a sufficiently good conductor of electricity to allow the temperature of the molten bath not to reach much above the melting point of the metal. In the Heroult process they electrolyze alumina made fluid by the intense heat of the electric arc, at a temperature at which the metal has to be protected by being alloyed with copper or iron in order to retain it.

DETAILS AND ADVANTAGES OF THE HALL PROCESS.

In the practice of the Hall process the heat is retained in the molten bath by a covering of finely powdered carbon on the surface of the molten mixture. On top of this carbon raft the powdered alumina is placed, and when the voltmeter attached to each pot shows a rising resistance, the pot-tender stirs in more heated ore from the surface of the pot. The carbon soon comes to the surface, when a fresh supply of ore is placed on it. The feeding is thus easily made continuous, and as the electrolyte remains constant it only requires tapping the metal off — or, as is rather crudely but very satisfactorily done, dipping the metal out in cast-iron ladles, and skimming the electrolyte back into the pots with the carbon rods — to make the entire operation continuous. The pots are run for months at a time. The following are the chief advantages of the continuity of the process :

1. *Purity of the Metal.*— After the first run of metal is cast, carrying with it all the iron and silica, or rather the reducible impurities of the electrolyte, the only other sources of impurity are in the pure alumina added and in the ash of the positive carbon electrodes, which are worn away in the proportion of a little less than weight of carbon to the weight of metal produced.

2. *Saving of Material.*— The loss of metal and ore in a finely divided state in the bath, which occurs when a shut-down is required, is avoided. To be sure, this loss can be largely avoided by re-melting again ; but in melting down the metal in a finely divided state it is found to be almost entirely re-dissolved. As continuously carried on, the loss of metal is practically nothing, every particle being reduced ; undoubtedly it is often reduced more than once by being re-dissolved before settling through the bath to the metal below, when the ore gets out of the electrolyte and it becomes acid. At the same time, as there is no slag or other waste product, solid or liquid, evolved, and as the gases can carry off none of the aluminum, all of the aluminum in the ore added is finally brought out in ingot metal, a result that is seldom experienced in the reduction of metals from their ores.

3. *Economy.*— The original heat of the electrolyte is constantly maintained, and the only supplies needed are the constant electric current, the alumina ore and fresh carbon electrodes to replace those worn out, with only occasional additions of fresh electrolyte to replace

the small loss of that taken out with the ingot metal or that which has become decomposed by the workmen carelessly letting the pots get out of ore. This decomposition of the electrolyte means an infusible "cake," as the workmen call it, settling to the bottom of the pot and filling it up. When the pots are not skillfully run, or more easily decomposing electrolyte mixtures are used, this accumulation shortens the run of the pot, and also decreases the out-put. In the regular practice of the Pittsburgh Reduction Company there is practically no decomposition of the electrolyte, and a pound of aluminum is made with an expenditure of about 22 electrical horse power per hour.

This electrical energy expended in heat is not very expensive. Supposing water power to be used at a cost of $12 per horse power per annum, allowing 25% for loss in converting mechanical into electrical energy, and supposing 75% of this energy to be converted into heat, Professor Richards has calculated the heat generated by 1 horse power during one year as 8,400,000 heat units.

The Hall process can be successfully carried on entirely independent of carbon, using a thick iron or copper tank, and either iron or copper electrodes. The deposition of the metal is nearly as large as with the use of carbon electrodes; but it is, of course, alloyed with copper or iron from the metal worn away from the positive electrode.

The Pittsburgh Reduction Company has made alloys of aluminum with iron and copper by this process similar in character to the alloys produced by the Cowles and Heroult alloy processes.

THE HEROULT AND MINET PROCESSES.

Almost at the same time that Hall invented his process for manufacture of aluminum, in the early part of 1886 (his first patent application dating July 9, 1886), M. Pierre Heroult commenced operations, and afterwards took out English patent No. 7,426 of 1887 for fluxing alumina with cryolite. The bath was put into a graphite crucible, which served as a negative electrode, and this was put inside a larger crucible, the space between the two being filled with graphite, and the carbon positive electrode being immersed in the fused bath. The ideas of M. Heroult and Mr. Hall at the start were very nearly identical. M. Heroult attempted to obtain an American patent, and was declared in interference with Hall; and, after the testimony as to dates of invention and of the application for patents, M. Heroult withdrew in

favor of Mr. Hall. This process was practically abandoned for a while by M. Heroult in favor of his very successful alloy process of electrolyzing and reducing molten alumina; but since 1889, when a growing demand sprung up for pure aluminum, and the success of the Hall process became known, I understand that two concerns have begun working under the same principles — the Aluminum Industrie Actien Gesellschaft, at Neuhausen, Switzerland, and a concern at Froges (Isère) in France, called the Société électro-métallurgique de France. I have seen some very pure and excellent metal from the Swiss concern.

The process commonly called "the Minet process," as developed and used at the works of the Bernard Brothers at Creil, Oise, France, consists in electrolyzing a mixture of sodium chloride with aluminum fluoride, or with the double fluoride of sodium and aluminum, their English patent dating July 18, 1887, No. 10,057. This company, I understand, have been doing successful work, and are now putting on the market aluminum of good quality. Thus it will be seen that there have developed apparently three separate electrolytic processes for the manufacture of pure aluminum at about the same time, all of which are working on about the same lines. These, together with the already much-written-about Cowles and Heroult processes for the manufacture of aluminum alloys, have for the past two years distanced all competitors manufacturing by the aid of metallic sodium, and are today in the possession of the aluminum market of the world.

FUTURE PROCESSES OF ALUMINUM PRODUCTION.

And now I will close with a few remarks upon the prospects and possibilities of the future — a subject in which, as President of the Pittsburgh Reduction Company, I am both financially and technically very much interested.

First, let me preface what I have to say by stating that I am not at all afraid of any new process of which the first we hear is the blast of trumpets and a general outcry through the newspapers of the very low cost at which the metal can be made, given in figures exact to a cent. I feel confident that either the inventor of such a process, or his friends, the company promoters, have so little sagacity and business sense that they will fail, even if they have a good thing; or else that they are simply attempting to further deceive the public and

prospective investors with delusive prospects, which, if well founded, would have been the very reason for keeping the matter quiet until they had become organized, and secured their capital and protection, so far as possible, by letters patent. If an inventor has a meritorious process for the manufacture economically favorable to the processes now in vogue, the method would have to be so simple that he would be able easily to prove it, and then he would have no trouble in securing capital to help him. The first thing any sagacious capitalist would then do would be to insist that cost prices be kept quiet, at least until it should be proved on an actual manufacturing scale just what cost prices were. For very often a most delusive difference is found to exist between the theoretical cost price and actual cost price, as may be proved by the books of almost any well organized manufacturing concern.

THEORETICAL COST OF MANUFACTURE BY ELECTROLYSIS.

Theoretically, the cost price of the manufacture of aluminum by direct electrolysis has already been brought down very low as compared with the cost of the more complicated processes of a few years ago. The cost per pound is about as follows:

2 lbs. of alumina (Al_2O_3, contains 52.94% Al_2) at 3 cts.	$ .06
1 lb. of carbon electrode at 2 cts.	.02
Chemicals, carbon dust, and pots	.01
22 E. HP. exerted one hour, water power being used	.05
Labor and superintendence	.03
General expense, interest and repairs.	.03
Total.	$ .20

I would say here that while the actual cost of the metal produced by the Pittsburgh Reduction Company, or any other of the concerns mentioned as actually putting aluminum in limited quantities on the markets today, are nothing like these figures as a total, at the same time, as the most economical methods are used, figures approaching these will undoubtedly be attained; and, moreover, improvements in new processes will in many cases probably be equally applicable in reducing costs by the present electrolytic methods. As stated before in this paper, no process can be devised that will give less loss of ore than that of the Hall process, where it is practically nil, nor

is it probable that any other method can be devised that is able to give purer metal than that made by the Hall process. Analyzing the items of cost given above, the ore will probably be the greatest item of expense in any successful process, and surely the oxide, alumina, is not only the richest and most easily prepared pure ore, but will prove one of the cheapest, if not the cheapest, of compounds from which to extract the metal. Quite surely no difference here in the art will make an insurmountable barrier of cost for the electrolytic processes now in use to compete against. The expenditure for other reagents than the ore, for carbon, and for chemicals, is now less than 5 cts. per lb., with the Pittsburgh Reduction Company, and in the estimate can fairly be reduced to 3 cts. per lb. for a large plant, with most favorable arrangements made for its supplies. In the item of electrical power there certainly may be room for a large curtailment of cost; but, even should this expenditure of electrical power be lessened one half, or entirely done away with, heat alone being substituted as the energy for reduction of the ore, it will be difficult to conceive of a method that would not require a cost of at least one cent for this heat, which would be a saving perhaps of 4 cts. per lb. upon this item of electrical power. However, I feel confident that should such processes be devised, the increased expenditure for chemicals and other reagents besides the amount quoted as necessary for the Hall electrolytic process will nearly, if not quite, counter-balance the saving in electrical energy expended in the Hall process.

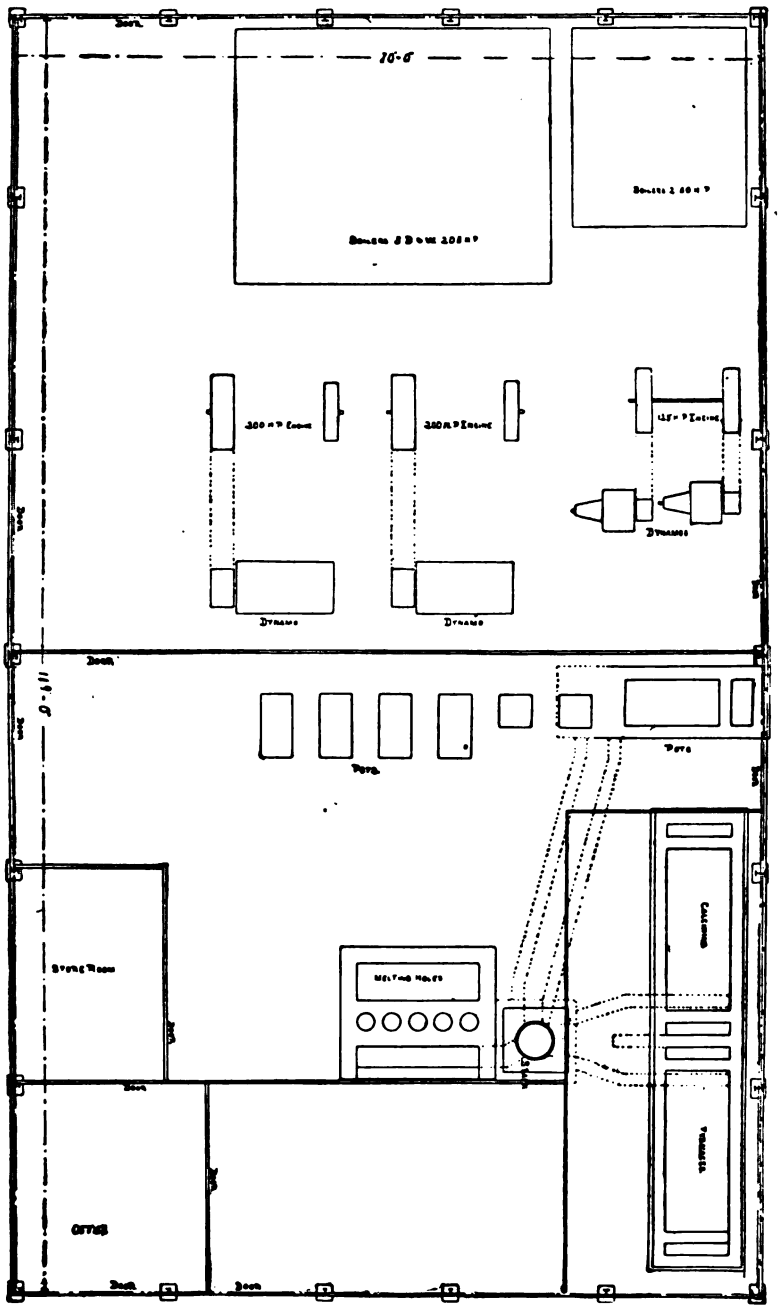
In the items of labor, superintendence and general expense, interest and repairs, there may be material saving made by a process yielding metal more rapidly than by the comparatively slow electrolytic process. But the plant required by the Hall process is very simple — especially if located upon water power — and the repairs are very small in proportion to the out-put. It will be doubtful, therefore, if a saving of more than 3 cts. per lb. can be made, or one half of the total estimated as necessary for the manufacture of aluminum by the Hall process. Thus the total saving probable, or, I think I am almost justified in saying, even in these days of wonderful inventions, possible, is only about 7 cts. per lb. over the cost of the Hall process. A greater saving than this may perhaps be made if some bright artist shall be able with Aladdin's lamp to rub the clay bank with his magic wand and extract "the silver from the clay" by his incantations; or, perhaps,

more soberly, some one may find a method of reducing aluminum from some of its compounds as a by-product, the expense of which shall entirely or mainly be borne by the other valuable products made.

POSSIBLE COMPETITORS OF THE ELECTROLYTIC PROCESS.

In this connection, an ingenious method has been devised to electrolyze the sulphide of aluminum, or the double sulphide of aluminum and sodium, in a tank, closed and suitably insulated, and connected with a powerful electric current. The tank is filled with charcoal, covered with alumina, or, in case the double sulphide is to be made, with a mixture of alumina and some sodium salt. The tank is heated to incandescence by the electric current, and fumes of sulphur are forced into the tank. An atmosphere of bi-sulphide of carbon is produced; the excess is carried away by a condensing pipe, and is calculated to yield revenue enough to pay for the entire operation, which consists further in reducing the sulphide of aluminum produced by a portion of the bi-sulphide of carbon acting upon the alumina in the presence of the heated carbon. The sulphide of aluminum is electrolyzed in the lower part of the tank, the molten metal going to the bottom, where it is afterward tapped out, The evolved sulphur again attacks the hot coal and produces bi-sulphide of carbon. This process looks very pretty on paper, but, unfortunately, it has not thus far proved financially successful.

The only method which seems at all likely to supersede the electrolysis of alumina as the most economical method of manufacture of aluminum is some variation in the condition of reduction (such as performing under pressure or the like). The oxide, or an equivalent aluminum compound, would have to be cheaply prepared, by the aid of carbon at a much lower temperature than is now possible by the means already tried. If such a feat could be accomplished, the changed conditions under which the carbon was to work not being too expensive to attain, and the reduction being accomplished rapidly enough to admit of the process being used upon a large commercial scale, then the electrolytic methods of manufacturing aluminum would have to be relegated to the same fate that has already overtaken the sodium method of reduction. But, in the light of what I have already said, I venture here to predict a long life and a happy one yet for the electrolytic methods of manufacture of aluminum.



PLAN OF THE PITTSBURGH REDUCTION COMPANY'S WORKS.

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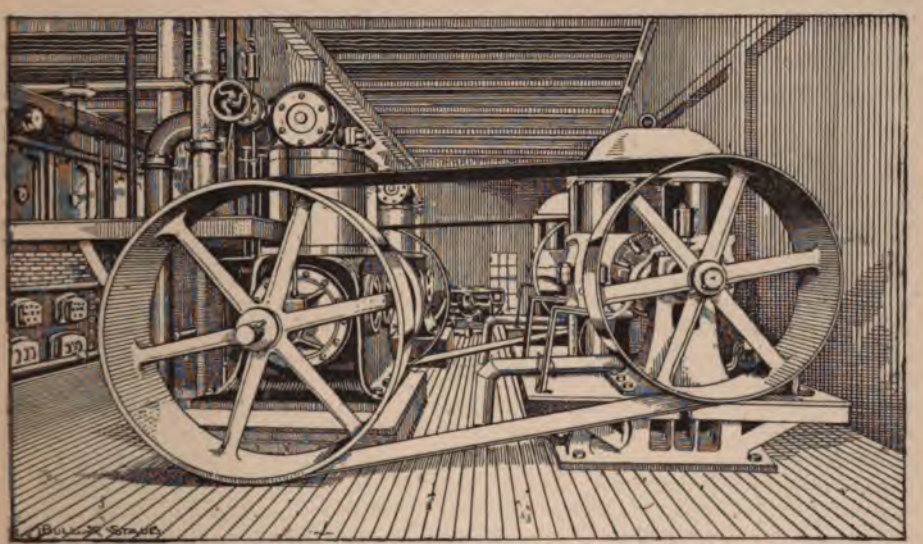
In closing, I wish to acknowledge my indebtedness to Professor Joseph W. Richards, of Lehigh University, not only for personal statements and suggestions, which I have embodied in this lecture, but also for data from his admirable book on aluminum. It is the first work on the subject printed in the English language, and I prophesy that it will always hold an authoritative position among the best treatises on the metallurgy and properties of aluminum, a place akin to that held by Percy's "Metallurgy of Iron and Steel."



DYNAMO ROOM — PITTSBURGH REDUCTION COMPANY.

ON THE CONTRAST IN COLOR OF THE SOILS OF HIGH AND LOW LATITUDES.

By W. O. CROSBY.

THE general contrast in color of Northern and Southern soils has attracted my attention for many years; and six years ago I suggested an explanation of this difference, which is evidently due to the condition of the ferric oxide, in a communication to the Boston Society of Natural History,[1] from which several paragraphs may be advantageously quoted as an introduction to the present paper:

The prevailing difference in color between the soils of the North and South is an unquestionable fact, and must be familiar to many travelers; and yet, but few geological writers have even mentioned it, and, so far as I can learn, no explanation of it has heretofore been proposed. In all latitudes the most superficial detritus, the true agricultural soil, is, in a large measure, distinctly carbonaceous, or the organic matter has at least been sufficient to more or less completely discharge the brown, yellow, and red colors due to the ferric oxides. But in the surface soil to a considerable extent, and in the subsoil generally, the ferric oxides are still the predominant coloring agents. Now throughout the Northern States and Canada the soils, where their colors can be ascribed to ferric oxide, are generally, almost universally, brownish or yellowish, but not distinctly red. The only important exceptions are where the red soil results from the disintegration of a red rock, or is itself geologically old. Thus, the red color of the soil on the Triassic areas, and of the clays at Brandon, Vermont, and Gay Head, does not belong to the present or any recent period, but is due to the peroxidation of iron in Triassic and Tertiary times. On the other hand, one of the most striking features of the scenery of the Southern States, especially for Northern eyes, is the bright red color of the soil, and the general predominance of this color over the brownish and yellowish tints. This begins to be noticeable in the latitude of Southern Pennsylvania, and becomes more and more marked as we cross Virginia into the Carolinas; while in the West Indies and South America the redness of the soil is even more intense and universal than in the Southern States. So far as I have been able to learn by reading and inquiry, this difference in color between the soils of high and low latitudes is more or less distinctly observable in all longitudes, and in the southern as well as the northern hemisphere.

The brown, yellow, and buff colors, so characteristic of Northern soils, are undoubtedly due chiefly to the yellow ferric hydrates, like göthite, limonite, and xanthosiderite; while the red color of Southern soils, although commonly attributed to hematite, is probably in many, if not most, cases due to the red ferric hydrate, turgite. The main question, then, Why are Northern soils yellow and Southern soils red? is really equivalent to, Why is the ferric oxide in Northern soils highly hydrated (göthite, limonite, etc.), while that in Southern soils is only slightly hydrated (turgite), or anhydrous (hematite)?

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[1] Proceedings of the Boston Society of Natural History, Vol. XXIII, pp. 219–222.

It is manifestly impossible to answer this question by correlating the difference in color with a difference in the rocks of the two regions; for, while the red clays of the South are found on nearly all geological formations, they appear to have their best development on the primary or crystalline rocks, and these are indistinguishable from the similar rocks of the North. But a satisfactory solution is, I think, found by correlating the color-difference with the one physical feature upon which all the other contrasts between the North and South depend — the climate. In other words, the difference in color depends upon a difference in temperature. It is well known to chemists that ferric hydrate, the coloring agent of Northern soils, is dehydrated at the temperature of boiling water; and it seems probable that a partial, if not complete, dehydration may result at much lower temperatures, if unlimited or geologically long time is allowed. And, in this connection, it is important to observe that the surface soils of the South attain at times a high temperature, and that in both regions, but especially in the South, the detritus is, quite certainly, chiefly of preglacial origin. The detritus of the South, it is well known, is, except on the flood-plains of the streams, chiefly sedentary, often retaining almost perfectly the structure lines of the rock from which it is derived; while the débris covering the rocks in the North is almost wholly transported, consisting of the modified and unmodified glacial drift. Hence it is evident that the characteristic colors of the North and South are approximately coterminous with the sedentary detritus and drift. But it seems impossible to ascribe the color-difference to glaciation; for wherever in the North we find sedentary soils, either post or ante-glacial, as in the case of trap dikes which have been decomposed to a considerable depth below the surface of the inclosing rocks, the colors are brown and yellow, never red.

Although it seems not to have attracted general attention, my observations show that frequently, if not always, the red color of the Southern soils is a merely superficial phenomenon, being most strongly marked at the surface, and gradually changing to yellow at a moderate depth. This fact, if fully established, will strongly corroborate the view here proposed, that the solar heat is the principal cause of the dehydration of the ferric oxide.

Nearly all soils originate, directly or indirectly, in the decay of the silicate minerals of the crystalline rocks, in which the iron is very largely in the ferrous state. And it is well known that the meteoric waters percolating through the rocks not only introduce the carbon dioxide, which is the chief agent in the kaolinization of the anhydrous silicates, but also the free oxygen required for the peroxidation of the iron. The sedentary soils of the South show very plainly also that the second process does not keep pace with the first; for, while the superficial soil exhibits the brilliant colors of the ferric oxides, in the lower portion, which shades off insensibly into the underlying rocks, grayish and bluish tints prevail, indicating that the iron is still chiefly in the ferrous state. Hence the normal vertical order of colors in sedentary detritus seems to be as follows, beginning at the base: 1, Bluish, grayish, and neutral tints, due to ferrous oxide; 2, the yellow and brown tints of the ferric hydrates; and, 3, in warm countries, the red resulting from the dehydration of the ferric hydrates.

The foregoing embodies the chief points of my previous paper, which was based very largely upon personal observations made some twenty years ago while I was engaged in mining operations in North Carolina and Virginia, and enjoyed unusually favorable opportunities for observing fresh and normal sections of the sedentary soil. Later, also, I travelled extensively in these and other Southern States; in the West Indies, from Cuba to Trinidad and the northern coast of South America; and in Europe, from Sicily to Scandinavia — giving particular attention everywhere to the colors of the soils. That paper was pub-

lished in the hope that it would call out the observations and views of other geologists; but it appears to have borne no fruit of that kind until the appearance, a little more than a year ago, of the comprehensive and valuable monograph by I. C. Russell on the "subaërial decay of rocks and origin of the red color of certain formations,"[1] to which the author has appended an important bibliographic list. In the first part of this essay, relating to the subaërial decay of rocks,

> Attention is directed to the widespread decay of the surface rocks throughout the Appalachian region, south of the southern limit of the glaciated area of Northeastern America. It is shown, also, that rock-decay is far more advanced in the Southern than in the Central and Northern Atlantic States, and increases gradually southward. This variation is thought to be due to climatic causes, combined with recent orographic movements which have accelerated denudation in the northern portion of the region under discussion. From a brief review of the geographical distribution of residual deposits in various regions, together with other considerations, the conclusion is drawn that rocks decay most rapidly in warm, humid climates.

After describing the characteristics of the residual or sedentary clays of the South, including the color, Mr. Russell refers to my contribution as follows:

> The contrast in color between northern and southern landscapes in the Appalachian Belt has recently been explained by W. O. Crosby . . . on the assumption that the observed difference of color has resulted directly from differences of temperature between the North and South. In the essay referred to it is urged that the higher temperature at the South is capable of dehydrating the ferric oxides impregnating the soils, thus changing their color from yellow to red; and also that the red color of residual clays is a superficial phenomenon, confined to the immediate surface of the deposits. My own observations do not confirm these conclusions. At many localities in the Appalachian region south of Pennsylvania where residual deposits were observed, the characteristic red color was seen to extend far below the surface, and as a rule to reach the bottom of fresh exposures. In many localities the color of the residual clays at a depth of twenty or even fifty feet is similar, so far as the eye can distinguish, to the color in the same sections only a foot or two below the surface. The great depth to which the red color extends renders it evident that it cannot be directly dependent on solar heat. Again, over considerable areas in the South the surface clays are various shades of yellow, which would not be expected if the red color of adjacent fields is due to temperature.

During the last six years I have gradually come to attach less weight than at first to the difference in temperature as a sole and sufficient cause of the difference in color between Northern and Southern soils; but I still hold that it is an important factor in the entirely adequate explanation; and so, apparently, does Mr. Russell. In fact, he proves that the cause must be largely climatic by showing: (1) that

[1] Bulletin No. 52 of the United States Geological Survey, Washington, 1889.

the typical residual clays of the South are usually red, and common to a great variety of geological formations; and (2) that, as already quoted, they are conditioned in a very large degree by the combined action of heat and moisture, being but scantily developed in the arid regions of the South, and never with a red color under the humid climate of the North. Since the rocks from which the red clays are derived are almost never red, the redness is evidently incidental to the kaolinization, and its cause may therefore be looked for with much probability, at least, among the conditions favoring kaolinization, of which heat is clearly one of the most important. Again, in spite of the recent glaciation, evidences of kaolinization are not wanting in the North; but the resulting clay, the true sedentary soil of the North, is never red, indicating very plainly that, while a warm climate is not strictly essential to the formation of residual clays, it is a necessary condition of the development of the red color. Prof. J. D. Dana emphasizes this point in a review[1] of Mr. Russell's work, from which a few sentences may be quoted:

> The contrast in colors between the Northern and Southern States is spoken of (by Russell) as "a contrast between a glaciated country and a region in which atmospheric decay has progressed uninterruptedly for ages." Mr. Russell, knowing less of New England than of other parts of the country, does not appreciate as strongly as Professor Crosby, whom he criticises, the full character of this difference. There is wide decomposition at the North, and its rapid progress in the case of syenites, mica schists, gneisses, granites, and hornblende schists, during the past forty years, is very strikingly exhibited alongside of many railroad cuts. The fact to be accounted for is that these decompositions over New England, whether in the trap of trap dikes or in metamorphic rocks, produces almost *never* red earth; while at the South, red earth predominates. The glacial movements and orographic changes have nothing to do with this. The fact is simply that in New England the result of the iron oxidation attending the decay is limonite, the hydrous, yellow-brown iron oxide, and not anhydrous Fe_2O_3. The writer has often tried to discover a reason for the different result at the South; he does not find one in Mr. Russell's excellent paper.

In what manner, if any, the milder climate of the South promotes the development of the red color, except directly by favoring the dehydration of the iron oxide, I have not attempted to explain; but when we consider that, aside from the glaciation of the North, there is virtually or primarily but this one physical contrast between the North and South, it seems impossible to doubt the existence, directly or indirectly, of a causal connection between the temperature and the color of the soil. The fact that Mr. Russell questions this conclusion caused me, however, less surprise than his attempt to fortify that negative position

[1] American Journal of Science, Volume XXXIX, 1890, pp. 317–319.

by denying that the red color of Southern clays is essentially a superficial feature. I was amazed at his statements in this regard, and asked myself again and again if my recollection of what I had seen in the gold mines of North Carolina and elsewhere could possibly be so far astray. Being unwilling to controvert the views of so competent an observer without a fresh examination of the facts, I have waited for an opportunity to go over the ground again. Fortunately, I have been able during the past year to make two journeys in the South — one to Asheville, by the usual route through the Piedmont district of Virginia and North Carolina, and the other down the great Appalachian Valley to the vicinity of the Natural Bridge — giving, in both cases, especial attention to the distribution of the colors in the superficial detritus.

Briefly stated, the general result of these recent observations is a complete confirmation of my original views; but I am able now to make more definite statements than formerly. In the great majority of the sections observed in the Piedmont region of both North Carolina and Virgina, including all which could be described as normal examples, *i. e.*, as unmodified by disturbance or erosion, the distinctly red soil is very superficial, varying in thickness, as a rule, only from two to five feet, and very rarely exceeding ten feet. It is usually reddest at, or near, the surface, changing downward gradually, more rarely abruptly, through various shades of orange to yellow; while occasional complete sections show the yellow changing through paler tints to gray or the color of the underlying hard rocks. This is certainly the normal succession of colors in a complete vertical section, as described in my earlier paper. The slight depth — two to five or ten feet — reached by the red color was noted again and again in scores of sections. It was usually easy to see, however, how hasty or unguarded observations might lead to a different conclusion; for the wash of the rains has, in most cases, carried the superficial red clay down over the entire face of the section. In this way the orange and yellow are often almost completely blotted out, except where an occasional gully, one to several feet in depth, notches the face of the cutting and exposes a clean, fresh, undisturbed, vertical section of the clays. When passing through the railway cuttings my eyes were always focussed upon these gullies or miniature ravines; and when on foot I have proved by actual digging that the yellow color seen in the middle and lower part of the gullies is strictly *in situ*, and that the red color at the same levels

between the gullies is what it appears to be, a mere surface wash —a red veil descending from the red crown at the top of the section.

For the past twelve years I have had in my teaching collection a series of specimens illustrating the normal gradation of colors from the surface soil to the underlying rock, which were carefully selected for this purpose at a gold mine in Fluvanna County, Virginia; the entire section, from the surface to the unaltered mica schist, measuring, in this instance, less than 12 feet, and the red clay less than 3 feet. More recently, I have collected a similar series in the vicinity of Rock Creek, in the District of Columbia.

In connection with his criticism of my views, Mr. Russell refers to a quotation, on an earlier page of his essay, from an account by Prof. E. A. Smith of the residual clays of Alabama.[1] Professor Smith's description, so far as it is pertinent to the present discussion, is as follows:

> The soil of the red lands is derived from the decomposed hornblendic gneisses and slates, which in many places, where exposed in washes or gullies, are seen to be mere stratified clays, containing fragments, more or less angular, of the quartz veins or seams, which are nearly always interbedded with the other rocks of this region. The top stratum of this soil, from two to three inches in depth, has often a dark chocolate-brown color, but below it becomes a bright red, and at varying depths, from ten to fifteen feet, becomes a *yellowish, hard clay. Where the freshly decomposed rocks are seen the color is yellowish rather than red, the latter color (red) being darker and more intense, apparently, the further removed the soil is from its original position* and the more it is affected by the decay of the vegetable matter.

The italics are my own; but how this description lends any support to Mr. Russell's criticism is certainly not very clear; on the contrary, it corroborates my statements that the red color is limited to the vicinity of the surface, and that the deeper and newer clay in every normal section is yellow. Professor Smith's description (which would apply equally well to large areas in Georgia) indicates, what might naturally be expected from the lower latitude, a greater average thickness of the red clay than I have observed in North Carolina and Virginia.

Whatever the cause of the superficial dehydration of the ferric oxide, changing the color from yellow to red, it is unquestionably a slow process; and since the red portion of the soil is clearly the oldest part, residual, unlike sedimentary, deposits growing from the top downward, we may find here an easy and sufficient explanation of the point

[1] Geological Survey of Alabama, Report for 1881 and 1882, p. 184.

in Mr. Russell's criticism to the effect that "over considerable areas in the South the surface clays are various shades of yellow, which would not be expected if the red color of adjacent fields is due to temperature" (page 42). We have only to suppose that erosion, which acts upon all areas in some degree, is here sufficiently rapid to prevent the development of the red color, removing the clay before it has time to change its hue. Mr. Russell has pointed out very clearly that the simple existence of the residual clays proves a general predominance of decay over denudation; but even where the rate of decay is uniform, the ever varying conditions of erosion must give rise to every phase between the greatest observed depth of undisturbed residual clay with the full complement of colors — red, orange, yellow, and gray — and the hard, bare ledges seen in crags and stream beds; and we may safely predict that in passing from the one extreme of denudation to the other, these tints will, as a rule, appear in succession at the surface.

According to my observations, the surface of the strictly sedentary detritus of the Piedmont region is rarely yellow, except where the conditions are obviously more favorable for rapid erosion than over adjacent red areas. Often in the same limited field it can be seen that the steeper slopes of the land, or, in general, those areas most exposed to the wash of the rains, are yellow, while the more level or less exposed parts are red. The colors thus tend to distinguish the areas of slow and rapid erosion; but it is intended, of course, to embrace in the latter, as in the former, only the general ablation of the surface, and not the gullies so characteristic of Southern hillsides, which, when once started, quickly cut clean, vertical sections through the clays. Now the fact that the red clay washed by the rains from the steeper slopes must be spread, in large part, over the more level areas immediately adjoining, affords an obvious and simple explanation, not only of the constantly varying thickness of the red clay, but especially of the exceptionally great thicknesses sometimes observed. I have purposely neglected to take account of these before, because the statement that the red clay is mainly superficial was intended to apply only to detritus that is still *in situ*, or strictly sedentary. No argument is required to show that by rain-wash from surrounding slopes the red clay might be accumulated upon a limited area to almost any depth, even fifty feet, as stated by Russell. But it is an obvious mistake to

compare such special accumulations of transported detritus, which are in general readily recognized by their situations and horizontal stratification, with clay which is still *in situ*. The same principle also explains the exposure of yellow clay over level areas; for evidently when the red clay has been completely washed from the slopes, the yellow clay will experience a similar ablation; and it is not difficult to see how the conditions would often be favorable to a commingling or interstratification of red and yellow clays.

On passing from the Piedmont district of North Carolina to the Blue Ridge and the table-land beyond, the red clay, through the accelerated erosion, rapidly disappears, except in the most protected places. The outcropping ledges and crags are bordered by successive and perfectly blended zones of disintegrating rock, and gray, grayish yellow, and yellow clays; and only, as a rule, on the gentlest declivities are orange and red tints observed. The same conditions were observed in the Appalachian Valley, between the Pennsylvania line and the Natural Bridge, yellow and orange being the prevailing colors, and red rarely occurring in considerable patches. Although the comparative absence of the red clay in these more elevated and mountainous areas might be attributed to the colder climate, the correlation with the accelerated erosion appears on the whole more simple and direct; and the latter must certainly be regarded as at least the chief cause.

Although it appears unnecessary to materially modify my previous statements concerning the distribution of the colors in sedentary detritus, I am, as already stated, less disposed than formerly to insist upon the entire adequacy of the warmer climate of the South as an explanation of the red surface soil of that section.

The dehydration of the ferric oxide is not wholly dependent upon heat or pressure or any obvious extraneous agency, but it is in a large degree, apparently, a spontaneous process. Of this we have abundant evidence in nature and in the laboratory. When the iron, which exists in the various silicate minerals chiefly in the ferrous state, is liberated and peroxidized during the decay of these species, it combines naturally with a very large and indefinite proportion of water, forming the yellow hydrate which is seen as a flocculent or a gelatinous colloid in the waters of springs, bogs, and marshes, and when the hydrate is obtained as a precipitate in the laboratory. But this colloid mass, even if immersed in water and entirely undisturbed, gradually and spontaneously

gives off a large part of the water which the ferric oxide has so greedily absorbed when in the nascent state; and it appears thus, as it slowly solidifies and hardens, to pass in succession through the forms of the various native yellow hydrates — limnite, xanthosiderite, and limonite, to göthite. That this progressive change continues is evident from the fact that these yellow hydrates are gradually replaced in the older formations by the red hydrate (turgite) and by ferric anhydride (hematite). When occurring as original or contemporaneous, and not as secondary, deposits, the yellow ores of iron are found, as a rule, only in the later rocks; while the red ores are very generally restricted to the earlier rocks. This genetic relation of the yellow and red ores is one of the most familiar and generally accepted facts in geology. However recent the origin of the red ore (turgite or hematite) may appear to be in any case, we naturally infer that it was first yellow, and that it has passed slowly or rapidly, as the case may be, but gradually, through the series of yellow hydrates.

The frequent absence of any apparent cause for this change leads us to assume that it is essentially spontaneous, in the sense that, although often hastened by heat or other extraneous agency, it would take place eventually without such aid. This view is strengthened by the analogous series of changes exhibited by silica. The gelatinous silicic hydrate obtained in the laboratory or seen in the waters of thermal springs, loses water and hardens spontaneously, and eventually reaches the comparatively stable condition of opal, which is comparable with limonite; and since opal, like limonite, is always of recent origin, we know that it must change more slowly into anhydrous silica or quartz, as limonite changes to hematite. The aluminum and other hydrates manifest a similar tendency. As the dehydration continues, there is a concomitant change from the amorphous to the crystalline state, analogous to that observed in the devitrification of glass and obsidian; and the dehydration is probably as spontaneous as the crystallization.

If it be conceded that the dehydration is virtually, if not absolutely, spontaneous, and there is no apparent alternative, it follows that the color of a deposit, so far as it is due to ferric oxide, is, other things being equal, a function of its geological age. In other words, the color naturally tends with the lapse of time to change from yellow to red; and, although this tendency exists independently of the temperature,

it is undoubtedly greatly favored by a warm climate. Applying this principle to the sedentary soil of the South, we find that the superficial portion is red, not alone because it is exposed to a higher temperature than the subjacent yellow clay, but also because it is the oldest part. On the other hand, the limited occurrences of post-glacial sedentary detritus in the North are, in the absence of the favoring climatic influence, still too young to exhibit the change of color even superficially.

It is generally conceded that the glaciated area was, in preglacial times, covered with a continuous sheet of sedentary detritus similar to that of the South, but probably not so thick. This now forms a large part of the till or bowlder-clay; and in its colors, thoroughly mixed and greatly diluted with gray (triturated rock), we have the color of the till. A simple experiment shows that a small proportion of the red clay of the South mixed with gray clay gives a decided reddish tinge to the whole. The absence of this reddish tinge in the till may, however, be at least partly explained by the strong erosion attending the preglacial elevation of the land; on the same principle that the red clay is now mainly wanting on the mountains of the South. It seems, however, impossible that a thickness of red clay in the North comparable with what now exists in the South could have been so completely swept away or extinguished as the character of the till would indicate. Hence we naturally fall back again upon the alternative view that the red color was developed very scantily, if at all, in the North, in preglacial times, and that, after all, the climatic difference is an important factor in the true explanation of the contrast in color between the residual clays of the North and South. Certainly no other explanation accounts so satisfactorily for the fact that in low latitudes flows of basaltic lava assume in a few years a bright red color, which never happens in the North. The general conclusion, then, to which the foregoing considerations lead is that the color-contrast is due chiefly to the difference in climate, but that the operation of this principle is modified in a general way by the essentially spontaneous tendency of the color to change from yellow to red.

IRRIGATION IN ARIZONA.[1]

By F. H. NEWELL.

THE total area in Arizona on which crops were raised by irrigation in the census year ending June 30, 1890, was 65,821 acres, or 102.8 square miles, less than one tenth of one per cent of the entire area of the Territory. The aggregate number of farms was 1,448, and of these, 1,075, or 74%, depended upon irrigation, the remaining 26% being stock ranches, or farms situated high in the mountains, where crops can be raised by what is known as "dry farming." The average size of irrigated farms, or rather of the irrigated portions of farms on which irrigation was practiced, was sixty-one acres. In this connection the term "irrigated farm" is used to include only the area on which crops were raised by irrigation, the uncultivated portions of such farms not being taken into account. With this understanding, the irrigated farms or areas cultivated by each person have been classified as follows: seven irrigated farms of 640 acres or upwards, 15 of from 320 to 640 acres, and 57 of from 160 to 320 acres. These 79 farms contain an average of 287 acres each, and have a total area of 22,656 acres, or 34% of the entire amount watered in the Territory. The remaining 996 farms, under 160 acres in size, comprise only 66% of the total irrigated area, and average 43 acres each. In other words, 7% of the farmers of the Territory owned over one third of the productive land, and the remaining 93% of irrigators owned an average of 43 acres each.

[1] Census Bulletin No. 35, Feb. 27, 1891.

COUNTIES.	Number of irrigators.	Total irrigated acreage in crop.	Average size of irrigated farms, in acres.	Average value of products per acre.
Total	1,075	65,821	61	$13.92
Apache	182	5,545	30	$13.36
Cochise	52	2,372	46	26.93
Gila	18	815	45	23.85
Graham	199	7,556	38	16.06
Maricopa	327	35,212	108	9.26
Pima	85	3,085	36	30.36
Pinal	115	6,919	60	11.25
Yavapai	91	3,762	41	31.00
Yuma	6	555	93	10.50

Taking all the counties in the Territory, with their varying conditions, the average cost of water right was $7.07 per acre. This includes all cases, from the one extreme, in which the farmer dug his own ditches from the river — the cost of water right in that case being the amount which the ditch cost him in labor and material — to the other extreme, in which he purchased his water right from some company, paying a certain sum per acre for the privilege of renting or buying water each year. The selling value placed upon this by the farmer, whenever his right was transferable, was $12.58. The average annual cost for water was $1.55 per acre, this expenditure being either in labor in keeping the main ditches and dams in repair, or paid as a cash assessment or rental to an association or corporation.

Besides the first cost of the water and the annual assessment for its use, an estimate has been made of the first cost of bringing the land from a wild state under cultivation by irrigation beyond the expense for the water right. This estimate, which is $8.60 per acre, includes the cost of ploughing, grubbing sagebrush, cutting mesquite, fencing, and levelling, or otherwise preparing the ground for irrigation.

Assuming, then, that the original purchase price of the land was $1.25 per acre, the cost of preparing for cultivation by irrigation

$8.60 per acre, and the first cost of water right $7.07 per acre, the land cost the original owners a total of $16.92 per acre. It is ascertained, for comparison with this, that the average valuation, including buildings, fences, and other improvements, of the land on which irrigation is practiced, is placed by the owners at $48.68 per acre, showing an apparent profit, less cost of buildings, of $31.76 per acre.

In comparison with the annual outlay for water it is found that the average annual value of farm products is $13.92 per acre, leaving the farmer $12.37 per acre per annum as a return for his labor and as interest on his investment.

On examination of the figures for the different counties, it will be noticed that in general the value of products per acre increases as the average size of the farm diminishes. For example, in Maricopa County the average size, 108 acres, is the greatest in the Territory, while the average value of product, $9.26 per acre, is the least. Next comes Yuma County, with an average of ninety-three acres per farm and products averaging $10.50 per acre; then Pinal, with an average of sixty acres per farm and products of $11.25 per acre. In other words, the larger irrigated farms were not so closely tilled as were the smaller, and a lower-priced crop was the result.

Deducting the 79 large farms, on which the average value of product was $9.05 per acre, there remain 996 farms, with an average extent of 43 acres each, on which the value of product was placed at $16.75 per acre; that is to say, the farmers, in the main, if their statements are correct, must have supported themselves and made their profits out of gross earnings averaging $720 per annum each.

These results have been computed from data acquired both by field work and correspondence. The enumerators in the several districts obtained the acreage, and likewise the value of land and products of each farm. On the completion of this field work a blank schedule was mailed to each farmer, requesting a more detailed statement as to the location of his land, the source of his water supply, the cost of irrigation, and also as to local customs. The replies were compared with the statements of the enumerators, and in every case of apparent misunderstanding or serious discrepancy a second letter was sent. The information thus obtained was then carefully examined in the light of personal knowledge of the cli-

mate and products of the Territory. Besides the blank schedule sent to farmers, correspondence was entered into with the owners or officers of canals, with a view of obtaining statistics as to cost of construction, etc. The result of this correspondence will be given at a later date.

The agricultural and irrigable land of Arizona is situated in the southwestern half of the Territory. If a line be drawn from the northwestern to the southeastern corner diagonally across the Territory, this line will lie for a greater portion of the distance along the face of the great escarpment which divides the high plateaus from the plains of the Gila basin. Against the face of this escarpment the larger portion of the available moisture is precipitated, rolling back to feed the tributaries of the Gila, thus rendering possible a certain amount of irrigation in the narrow valleys, especially on the Gila and Salt River plains near the point where their waters leave the cañons. High up on the edge of the plateau country and among the mountains, at elevations of from 5,000 to 7,000 feet, a little agriculture is successfully carried on without irrigation, usually in connection with stock raising, or in the immediate vicinity of mining towns. The crops raised, however, are comparatively insignificant.

The acreage at present under irrigation may be regarded as approaching the maximum possible with the present supply of water and methods of using it. In other words, all the easily available water has been utilized, and expansion can come only by a greater economy in the use of the existing supply, or by the adoption of systems of storage for the conservation of the flood water now annually running to waste. The irrigators look forward to the latter alternative as the most effectual means of obtaining relief from present troubles and uncertainties. Progress in this direction will, however, be slow, from the necessity of a large outlay of capital before any return can be realized, and from the fact that the controllers of capital, whether governmental or private, have had comparatively few facts on which to base reliable estimates. The amount of water that can be reserved by storage is known from common observation to be very large, but it is usually greatly overestimated, from the fact that even a small volume of water, coming all at once and with great velocity, is often extremely destructive, and creates a vivid

impression, while the same amount, if distributed through as many days or weeks as it is hours, would not excite comment. Measurements have been made in a few instances by engineers and by the United States Geological Survey; but the operations of the latter were discontinued before a range of much over a year had been obtained. For example, taking what is probably the best storage site in Arizona, that on the Gila at the Buttes above Florence, the mean discharge at this place, as shown in the eleventh annual report of the director of the United States Geological Survey, from September 1, 1889, to August 31, 1890, when operations were suspended, was 503 second-feet, averaging 1,000 acre feet per day, or 365,000 acre feet for the whole year. Assuming a water duty of 100 acres to the second-foot, this, if all the water could be stored, would irrigate 50,000 acres, instead of the 7,000 acres or less now irrigated in the vicinity of Florence. A large reduction from this theoretical amount must, however, be made for losses by evaporation and seepage. Without going into a discussion of the measured water supply, this will suffice to show that it is possible to determine the volume and intensity of floods, and that when this has been done the water-storage problem will be in a fair way to solution, since only then will it be possible to prepare reliable estimates of costs and profits.

Shortage of water, and consequent loss of crops on irrigated land, are reported by the majority of farmers whose rights to water are secondary to those of the first comers. The latter, however, report that there is ample water for all their own needs, except in general from the middle or latter part of June, through July, August, and into September. Before this time, however, the cereals, with the exception of corn, should be matured, and probably two or more cuttings of alfalfa made.

For the greater part of the Territory it may be said that irrigation is practiced or is necessary to successful farming throughout the entire year. In the lower plains the temperature is such that crops will grow and mature at any season of the year, provided there is a sufficient supply of moisture. Crop follows crop in constant succession, if only there is an abundance of water in the ditch, or of summer rains. During the winter, and also in the early spring and late fall, water is used on the alfalfa and hay crops, from three to six cuttings of alfalfa being made annually, the average of the

entire Territory being five. Usually two waterings are given for each cutting, the total amount of water used being thus largely in excess of that required by any other crop. Later in the spring, when the rivers begin to decline, the alfalfa is neglected, all the water being needed for the cereals and vegetables, and still later, before the summer floods set in, in many places the entire flow has to be taken to save the trees and vines.

The water is in most cases applied by flooding — as, for instance, on alfalfa and grain — or through furrows laid off at regular intervals, no especial care being taken in the preparation of the fields. The Mexicans still practice the old method of flooding in small "pools" or rectangular areas, separated by ridges of earth. The farmers, almost without exception, look forward to better methods of distributing and applying water. Since the soil and local conditions vary so widely, there can be no absolute rule as to the number of waterings or the time required for the different products. There is a growing impression that in many cases too much water is used, and that better crops could be matured by using the water more sparingly, which would also have the effect of increasing the cultivable acreage.

Apache County occupies the extreme northeastern corner of Arizona, and borders upon New Mexico on the east and Utah on the north. The Navajo Indian reservation covers the northern half of the county, and the Atlantic & Pacific Railroad grants cross the southern half, thus leaving comparatively little of this vast area of 21,060 square miles open to settlement under the general land laws. The railroad just mentioned follows the Little Colorado River, and then turns up one of its tributaries, the Rio Puerco. The principal settlements are along this railroad and south of it on the Little Colorado and on Silver Creek, a tributary of this stream.

In these valleys, which are at a general elevation of from 5,000 to 6,000 feet, are long stretches of good agricultural land, which only need water to become very productive. The water supply, however, is limited, and although great quantities run to waste in time of flood, there is generally a deficiency during June and July, so that the acreage now under cultivation does not always receive its full share. There are reported to be good facilities for water storage near the head of several streams. Dry farming is practiced on

about twenty-five farms in the forest belt, and corn, wheat, rye, beans, potatoes, turnips, etc., are raised without irrigation in the vicinity of Show Low, the elevation here being 7,000 feet and upward. In the lower valleys, however, nothing can be grown without the application of water by artificial means.

As shown by the table, the total acreage irrigated is 5,545 acres. From the statements received from the farmers it is doubtful whether in ordinary seasons a larger acreage could be successfully cultivated without the use of stored waters. The principal ditches reported at this time are the East and West Taylor, East and West Snowflake, and the Woodruff, the total cost being $25,000, and the land furnished with water aggregating 2,500 acres. One of the most notable engineering features is the attempt to dam the Little Colorado River by a structure 225 feet long and 30 feet high. This has been washed out and replaced seven times.

The first cost of irrigation is reported to be from $4 to $12 per acre, the average being $7.50, while the average selling value put upon the transferable water rights is quoted at $9.65 per acre. The annual assessment, an expense for maintaining the ditches, varies from 25 cents to $4 per acre, averaging $1.60. Beyond the outlay for water, the general expense of preparing the ground for cultivation is $7.25 per acre, thus making the first cost of irrigating land $14.75 per acre, taking the county throughout.

Cochise County is in the southeastern corner of Arizona, bordering upon Mexico and New Mexico. In altitude it ranges from 3,000 to over 6,000 feet. The northern end of the county is crossed by the Southern Pacific Railroad, from which a branch line runs southward to Tombstone. There are in all 2,372 acres irrigated, the farms being confined almost exclusively to the valley along the San Pedro River. This stream is the main water supply of the county, the little farming that is done at any distance from the river being confined to the mountain valleys, where small springs occur.

There being far more arable land than river water, the latter is in great demand, and in many localities there is constant litigation concerning water rights, so much so that it may be said that the legal expenses are heavier than the cost of constructing ditches. During the periods of low water the entire stream is taken out by dams, some water, however, reappearing in the channel, to be diverted

in turn by a dam below. There are a large number of small private ditches along the San Pedro River, but there is no comprehensive system for utilizing or dividing the water. The lowest stage of water in this river is usually reached in June, when there is the greatest demand, and the rise begins again after the commencement of the summer rains, during which time the flood waters run largely to waste. All the land which the ordinary unregulated flow of the river will supply, and probably more than can be supplied in certain seasons, is now under cultivation, and yet the demand for agricultural products far exceeds the amount that can be raised. The storage problem will, however, have to be settled before any further advance can be made.

It is impossible to raise crops without irrigation, except in a very few localities in the mountain valleys, where, after the rainy season, small crops of corn and potatoes are successful. With irrigation, however, almost all the fruits and other products of the temperate zone are grown, with close cultivation one crop following another with but little loss of time.

Graham County lies to the southeast of Gila, between Apache and Cochise Counties, filling in the eastern side of the Territory. It is thus in or near the head waters of the Gila River, and, like Gila County, is largely mountainous. The principal area of agricultural land is in the Pueblo Valley, extending from a cañon above Solomonsville for about thirty miles to Fort Thomas, the principal settlements being, in order, Solomonsville, Safford, Central, and Pima. The Gila is the source of supply for the valley. A number of dams are placed across it at intervals of two miles or more. When the flow of the stream diminishes in May and June, the dams of logs, brush, and stone are tightened so as to turn all the water into the canals. Below each dam, however, the water begins to rise in the bed of the stream, so that by the time the next dam is reached there is apparently as much water as before. If the summer rains fail, the river may become entirely dry in July and August, and a loss of crops be the result.

At present the acreage under cultivation is not greatly in excess of the usual water supply, and the losses are not such as to call for immediate water conservation. In view of future extensions, however, it is already recognized that active steps must soon be

taken. There appears to be no comprehensive system of water distribution, and the present needs seem to point to a consolidation of the various small ditches into a few high-line canals, both to reduce the expenses and to effect a greater economy in the use of the water. The river, occupying a wide, sandy bed, is unstable, and during freshets is liable to cut new channels, leaving the dams on dry land.

Some cultivation by irrigation is successful in the valleys south of the river at the foot of the mountains, where small streams or springs issue. The greater portion of this water, however, is lost for lack of facilities for holding the spring flow until later in the season.

Gila County is the smallest in area in the Territory. It occupies a position largely within the drainage basin of the Salt River, on the south, crossing the mountains and reaching to the Gila River. On account of its position it is comparatively well watered, but on the other hand the valleys are narrow, and there is little arable land. The White Mountain Indian Reservation covers the greater portion of the county, leaving only a narrow strip of land on the western side. The tilled land is principally along Salt River, between Pinal and Tonto Creeks, or in their vicinity. Among the head waters of Tonto Creek and in the Tonto basin, at an elevation of from 6,000 to 7,000 feet, corn and potatoes are raised without irrigation if the land is carefully tilled. Other crops, however, require the artificial application of water.

Maricopa County leads in agricultural development, as well as in the value and extent of irrigation. Over one half of the irrigated land of the Territory is situated in this county, the amount, however, being small in comparison with the arable land which may in the future be brought under cultivation. The water supply is derived almost entirely from Salt River and from the Gila below the junction with the Salt. A large number of canals take water from both sides of the first-named river, their head works being at irregular intervals from each other, beginning near the Verde and continuing to Gila Bend. Statistics relating to these canals, some of which are among the most notable in the United States, will be given in a future bulletin.

Taking the county as a whole, it may be said that the water supply is ample for the acreage now under cultivation, except during

the latter part of June, and in July and August. Before this time of scarcity occurs, however, all the grain crops, with the exception of corn, should be matured, and probably two cuttings of hay obtained, so that the water can then be used to save the fruit trees and vines. On the older canals and ditches the farmers report that there is sufficient water, but on the newer systems there are many complaints against the exactions of the corporations and the amount of water and time allotted to each irrigator.

The need of better canal systems, of greater economy in the use of water, and especially of the storage of flood waters, is insisted upon by all irrigators. The enormous extent of good arable land and the volume of water going to waste every year seem to have impressed every resident of this county, and there is a unanimously expressed hope that either the national government or capitalists will in the near future begin the construction of reservoirs at one or more of the many favorable points. Storage in a small way has already been tried, and is reported to be successful.

Most of the existing dams and other contrivances for diverting water from the river to the smaller ditches are of such a temporary character as frequently to be partially or even wholly destroyed by floods, resulting in large expenditures of time and labor and in loss of crops. The ditches are all open, and their beds and banks are not lined, great loss by evaporation and seepage thereby necessarily occurring in so warm and dry a climate.

The inhabitants are enthusiastic as to the productiveness of the soil, and report that all the crops and fruits of temperate and semi-tropical zones flourish and attain a wonderful growth and perfection. The altitude varies from 500 to 2,000 feet, and the temperature is sufficiently high to insure the successful cultivation of cotton, sugar cane, and other Southern products.

Taking the county as a whole, the cost of water is $5.75 per acre, and the present value of this water right when transferable is quoted at $10.87 per acre, while the estimated cost of bringing the land under cultivation beyond the above expense is $6.45 per acre, making a total of $12.20 for the average outlay required beyond the purchase price of the land before a crop can be raised. The annual assessment or cost of cleaning and repairing ditches is estimated at $1.12.

The average size of irrigated farms in this county is 108 acres. Dividing them into two classes, those of 160 acres and upward and those under 160 acres, it is found that there are 64 farms of the first class, averaging 296 acres each, and 263 farms of the second class, averaging 62 acres each.

Mohave County occupies the extreme northwestern corner of the Territory. There is as yet no cultivation reported, cattle raising being the only agricultural industry.

Pima County is situated along the central part of the extreme southern border of the Territory, adjoining Mexico. Agriculture is confined to the eastern end, near Cochise County, where the elevations range from 2,500 feet upward. The Santa Cruz and Sonoita and their tributaries are the only sources of water supply, with the exception of a few springs among the foot-hills of the mountains. The amount of water is small in comparison to the acreage already under cultivation, and crops are frequently lost during the dry season.

Pinal County stands second in importance in regard to agricultural products. It lies between Maricopa and Pima Counties, and includes the most favorable portion of the Gila Valley; that is, the portion adjoining the foot-hills and cañons from which the river issues. The agricultural land is in the centre of the county, around and near Florence, with the exception of a small amount in the San Pedro Valley near Dudleyville.

The necessity of water storage is keenly felt in this county, from the scarcity experienced during a great part of the year. There are excellent opportunities for holding all the flood waters, and surveys have demonstrated the feasibility of the project, the only apparent obstacles being the lack of capital and of favorable legislation concerning the control of the water. The quantity of water in the Gila is reported to be decreasing on account of the large and increasing number of diversions on the upper portion of the stream, in Graham County.

The average cost of a water right is $6.94 per acre, and of clearing and bringing the land under cultivation, $11.50, making a total of $18.44 per acre. The annual cost per acre for water is estimated at $1.81.

Yavapai County, while the largest in area, being one fourth that of the entire Territory, contains relatively the smallest proportion of

irrigable land. This county lies in the northern half of Arizona, adjoining Utah on the north, and contains the greater portion of the grand cañons of the Colorado. These stupendous gorges, the most wonderful in the world, cut the great plateau to the depth of from 3,000 to 6,000 feet. The minor lateral cañons, in which flow the tributaries of the Colorado, are also cut to a great depth, which decreases toward their head waters. Thus the water of the northern part of the Territory, though large in amount, is wholly useless, lying as it does hundreds and thousands of feet below the level of the arable lands. It is only toward the southern portion of the county, where the great plateau begins to break off and the valleys are less deep and narrow, that agriculture has been seriously attempted. Along the line of the Atlantic and Pacific Railroad, which crosses the county from east to west at an elevation of from 5,000 to 7,000 feet, some crops, especially for forage, are raised without irrigation. For example, at Flagstaff, at an elevation of about 7,000 feet, corn, potatoes, and vegetables, as well as a little wheat, oats, and barley, are thus cultivated, the cereals being generally cut for forage purposes. The same is true of Prescott, although near that place irrigation is employed wherever practicable. On the head waters of the Agua Fria, at an elevation of about 4,500 feet, there is also a little dry farming.

The principal bodies of irrigated lands are on the Verde and its tributaries, Oak, Clear, and Beaver Creeks, above and below Camp Verde, where a large number of small ditches are owned by private individuals. On Walnut Creek, which heads west of Prescott and flows northward into Big Chino valley, and on Granite Creek to the east, the available waters are all utilized, and there is already need for the storage of the waste waters of the floods. The valleys generally are narrow and shut in by steep walls, and the present agricultural land lies in long, narrow strips, thus obviating the necessity for large canals. The best lands lie high above the water, and can only be reached, if at all, by expensive dams and headworks.

Yuma County, in the southwestern corner of Arizona, comprises the lowest lands of the Territory, and the drainage is consequently in that direction. The extreme lower border of the county is reported as possessing an almost tropical climate. The Colorado River forms the boundary between this county and California on the west, and

is navigable, small steamers running the entire length of that side of the county. There is a large amount of arable land lying along the river, but it is doubtful if dams can ever be placed in this great stream to raise its waters into canals. It is, however, not improbable that considerable land above the flood plains can be irrigated by means of steam pumps. In this climate, where fruits of the choicest and most profitable varieties can be raised, the profits will undoubtedly warrant this method of utilizing a portion of the water.

The Gila River flows across the southern end of this county, but, owing to the large number of canals deriving their water supply from this river in Maricopa County, there is comparatively little water remaining. A number of projects have, however, been started, and in a few years there will probably be a large acreage under cultivation along this portion of the river, although the amount now under cultivation is extremely limited.

ON THE DETERMINATION OF SOLIDS IN WATER.

By ALLEN HAZEN.

A LARGE number of experiments have been made during the last three years at the Lawrence Experiment Station with the use of sodium carbonate in making solid determinations, in the endeavor to make the loss on ignition show with some degree of accuracy the total amount of organic matter present in waters. While the attempts have not been entirely without success, the reduction of nitrate by the organic matter present, and the decomposition of magnesium carbonate, even at comparatively low temperatures, increased the loss, in the case of ground water and effluents, to a considerable extent. In surface waters with comparatively little mineral matter in solution, the sources of error from the above mentioned causes are very slight.[1]

The easier and more accurate determination of albuminoid ammonia and oxygen consumed from permanganate answered our purpose so much better that the determination of loss on ignition was finally abandoned.

It was shown by our experiments that the total solids obtained with the use of sodium carbonate, after deducting the weight of the soda used, is an accurate measure of the solid matter present — far more reliable than the simple residue of evaporation without soda.

If a solution of magnesium chloride is evaporated without soda, as in the usual course of water analysis, there is more or less hydrochloric acid lost, and the residue contains a large and variable amount of water of crystallization; so that the weight of the residue depends very largely upon the manipulation, and it would probably be impossible to proceed in such a way as to make the result correspond accurately with the amount of salt taken in every case. If,

[1] See paper on the Loss on Ignition in Water Analysis, by T. M. Drown, Technology Quarterly, December, 1888; Chemical News v. 59, p. 272.

however, an excess of sodium carbonate is added before evaporation, the magnesia is precipitated as carbonate, while the chlorine remains in solution as sodium chloride with the excess of soda. On evaporation to dryness there is not a trace of hydrochloric acid lost, and the residue contains no water of crystallization and is not deliquescent. Its weight is practically constant under widely differing conditions of manipulation, and, after deducting the weight of the soda used, represents accurately the weight of the anhydrous magnesium chloride originally present; for, in the double decomposition there is no change of weight.

The same can be said of all magnesium salts, and with calcium salts the results are even more exact, owing to the greater stability of calcium carbonate and the ease with which it is dried at 100°.

That this is practically the case is shown by the table which follows on the next page, of results with solutions of various salts evaporated to drynesss at 110°, and, after weighing, igniting in a radiator.

A sample of water, rich in salts but with little organic matter gave the following results:

	Without Soda.	With Soda.
Total solids	30.5	25.1
Loss on ignition	8.3	0.7
Fixed residue	22.2	24.4

Without soda the total solids are too high, on account of water of crystallization, and the fixed residue is too low, because a large part of the chlorine and nitric acid combined with the magnesia were lost on ignition. With soda, the total solids may be taken as an accurate measure of the solid matters present.

LAWRENCE EXPERIMENT STATION,
Massachusetts State Board of Health.

TABLE SHOWING THE RESIDUE OF EVAPORATION OF SOLUTIONS OF VARIOUS SALTS, WITH AND WITHOUT SODIUM CARBONATE.

SALTS TAKEN.	Amount Taken. Grams.	Total Solids.	Loss on Ignition.	Fixed Residue.
Distilled water	100	.0001	.0000	.0001
Distilled water with sodium carbonate, after deducting the amount added	100	.0004	.0000	.0004
Sodium carbonate	.0133	.0135	.0002	.0133
Sodium carbonate	.0265	.0270	.0000	.0270
Sodium chloride	.0250	.0248	.0000	.0248
Sodium nitrate	.0212	.0220	.0001	.0219
Sodium nitrate with sodium carbonate	.0212	.0217	.0001	.0216
Potassium sulphate	.0100	.0102	.0000	.0102
Potassium sulphate	.0250	.0250	.0000	.0250
Potassium sulphate with sodium carbonate	.0100	.0118	.0010	.0108
Potassium sulphate with sodium carbonate	.0250	.0266	.0006	.0260
Potassium nitrate	.0072	.0076	.0000	.0076
Potassium nitrate	.0200	.0204	.0003	.0201
Potassium nitrate	.0200	.0200	.0002	.0198
Potassium nitrate with sodium carbonate	.0072	.0071	.0003	.0068
Potassium nitrate with sodium carbonate	.0200	.0214	.0009	.0205
Potassium nitrate with sodium carbonate	.0200	.0209	.0005	.0204
Calcium chloride	.0101	.0134	–	.0146
Calcium chloride with sodium carbonate	.0101	.0103	.0000	.0103
Calcium chloride with sodium carbonate	.0338	.0347	.0003	.0344
Calcium chloride with sodium carbonate	.0338	.0340	.0003	.0337
Calcium carbonate dissolved in CO_2 water	.0480	.0492	.0003	.0489
Magnesium chloride	.0200	.0390	.0290	.0100
Magnesium chloride	.0200	.0362	.0262	.0100
Magnesium chloride	.0400	.0742	.0540	.0202
Magnesium chloride with sodium carbonate,	.0200	.0211	.0058	.0153
Magnesium chloride with sodium carbonate,	.0200	.0209	.0036	.0173
Magnesium chloride with sodium carbonate,	.0400	.0392	.0066	.0326
Magnesium chloride with sodium carbonate,	.0400	.0435	.0053	.0382
Magnesium carbonate in CO_2 water	.0113	.0136	.0052	.0084
Magnesium carbonate in CO_2 water	.0188	.0219	.0058	.0161
Magnesium carbonate in CO_2 water	.0376	.0427	.0156	.0271
Magnesium carbonate with sodium carbonate,	.0188	.0220	.0059	.0161
Sugar	.0050	.0065	.0050	.0015
Sugar with sodium carbonate	.0050	.0067	.0049	.0018
Sugar with .0212 gr. sodium nitrate	.0050	.0267	.0070	.0197
Sugar with .0212 gr. nitrate, and sodium carbonate	.0050	.0286	.0092	.0196

THE BEGINNINGS OF SCIENCE.

By GARY N. CALKINS, S. B.

In order to gain an outline of the history of scientific thought, a true comprehension of the spirit of the different times is necessary — of that spirit which inspired the great men of different ages to give us their immortal works.

It will be found that there is a cause for all things, and that all are interdependent. Thus the motive cause of the invention of trigonometry was the need of it for astronomical work, and this need was felt because of the desire of the Greek mind to know. But it is obviously impossible to seek the ultimate causes of all changes, and especially so in the time of the beginning of intellectual activity, when authentic history and mythology were so inextricably interwoven that all is obscure.

The development of the true scientific idea began among the most ancient of historic peoples. It is a period when man knew facts well enough for practical purposes, but did not understand the laws which govern them. As with art, science was at first used exclusively for religion. Mr. Herbert Spencer, in his "Genesis of Science," says: "This fact — that in very early stages of social progress it is known that the moon goes through her changes in about thirty days, and that in about twelve moons the seasons return — this fact that chronological astronomy assumes a certain scientific form even before geometry does — while it is partly due to the circumstance that the astronomical divisions, day, month, and year, are already made for us, is partly due to the further circumstance that agricultural and other operations were at first regulated astronomically, and that from the supposed divine nature of the heavenly bodies their motions determined the periodical religious festivals. As instances of the one we have the observations of the Egyptians that the rising of the Nile corresponded with the heliacal rising of Sirius; the directions given by Hesiod for reaping and ploughing,

according to the position of the Pleiades, and his maxim that 'fifty days after the turning of the sun is a seasonable time for beginning a voyage.' As instances of the other, we have the naming of the days after the sun, moon, and planets; the early attempts among Eastern nations to regulate the calendar so that the gods might not be offended by the displacement of their sacrifices; and the fixing of the great annual festival of the Peruvians by the position of the sun. In all which facts we see that, at first, science was simply an appliance of religion and industry."[1]

Under these conditions science could not rise to any great height; we see it bound down to the practical arts required by religion; but later an advance was made, when science, though still only for religion, rose from the practical side to the theoretical, as we see it in the philosophy of the Greeks. The next step was the development of science apart from religion, but, as before, it was first used for practical purposes only. Scientific facts were investigated, not for the love of science, but for their practical value. From this point we find men growing independent in their ideas, and with this independence, freedom in scientific thought. But today it is for the love of truth that men investigate Nature and her phenomena, and with this we have pure science.

We thus follow science in its development from pure sensation to the modern and highest form. At first, mere facts, gained by the impressions of the senses, were used for religious purposes; an advance was made when, though still for religious purposes, facts derived from the senses gave place to theoretical or abstract reasoning. Later religion and science were entirely separated, and science was cultivated solely for its practical value; but the highest point of its development is pure science, cultivated simply for the love of truth.

No one of these periods stands out by itself. All are bound securely together by that law of development which works constantly for the ultimate good of mankind and civilization. At the time when one spirit is shown by the greatest minds of the age, a lower spirit is shown by the lesser lights. When these latter have attained to the position of the former, the great minds are well into the next period of higher development. Thus today we see many people,

[1] "Illustrations of Universal Progress," page 165.

perhaps even the majority, cultivating science solely for its practical value; but for the greatest men of every age it is for the discovery of truth alone.

We have given as the first application of the scientific idea, the use which the most ancient people made of observed facts. But the spirit of observation and the distinction of like and unlike must have come earlier than this in order that these facts should be known to them. In fact, we may say with Lewes: "... the dawn of science is coëval with the dawn of human intelligence." Again: "Introduced to this mysterious universe naked and helpless, man is early forced to gain some understanding of its relations to him. At first all is dim sensation. To this succeeds a slowly organized classification of elementary experiences. The great forces of Nature are everywhere at work, everywhere manifesting themselves in manifold and intermingling motions, which he must appreciate, and intellectually disentangle, as far as they directly concern him. ... At first, man stands before the 'roaring loom of time' gazing in helpless perplexity at the movements of the infinite shuttles, ignorant of the movements which may be beneficent and of those which may be destructive to him. But he cannot remain thus. His necessities soon change this attitude of wonder into an attitude of inquiry." His bodily comfort is of primary importance; hence facts relating to man's physical nature are first ascertained. Then comes a desire for explanation, and thus an intellectual need springs up. He begins to question, and makes out a scheme, plausible to him, by which phenomena are bound together. He knows nothing but by his own feelings, so can explain only by the teachings of his senses, and explains what he sees and hears by what he feels and does.

From the store of knowledge, philosophy arose; meagre at first, but later giving rise to the grandest flights of the human imagination. The actively developing intellect, however, was not to be satisfied with an interpretation of extrinsic phenomena by intrinsic emotions. As the powers of observation improved, it was seen that phenomena agreed less and less with their former explanations. Doubts arose, and with these came a rearrangement of ideas, as well as an increase to the store of facts. These doubts mark the beginning of the first step in the development of science, whereby the practical application of facts gave place to theory.

In the history of human thought there have been two great methods of explaining natural phenomena. That which invariably comes first is known as the subjective method, and obtains today among the uncivilized races of the world. By it the savage is unable to get beyond himself in his explanations. To him the darkness of the night, the gloomy eclipse, the raging pestilence, and all phenomena which are not observed in the regular course of Nature, are the expressions of emotions similar to those which sway him. In other words, he explains Nature through himself.

By the second or objective method, man loses sight of himself and his emotions, and explains the causes of phenomena as inherent in the things themselves. A savage hears the ticking of a watch and sees the motion of the hands; these he can explain only by imagining the chronometer alive. But the owner of the watch knows the laws of force in coiled springs, and the action of cogged wheels, and, by this knowledge, he explains the ticking and the motion of the hands, and thereby shows the superiority of the objective over the subjective method. The old Nature worshippers saw in the sun, a warrior; in the moon, a huntress; and explained the lightning flash as a representation of some god's wrath. They saw in the wind one of the slaves of a god. In the beams of light shining through a rift in the clouds, they saw the sun, a god, drawing up water. Poseidon, the sea god, induces the winds to toss about the frail bark of Odysseus, until Ino comes to the latter's rescue and bids him strip and swim to the shore.

In this we see the homely, every-day life of a people who cannot get beyond themselves in their explanations of natural phenomena. How natural it is for man in this primitive stage to ascribe to a violent storm the same impulse that moves a man to strike his neighbor, when the latter has angered him. They do not conceive that force can originate in anything, except through some anthropomorphic agency. They see phenomena, and can explain them only by their daily habits and usages. "In this subjective method man claims direct knowledge of the nature of things and of the causes of all changes." (Lewes.)

In direct contrast to this is the objective or modern method. Fetiches were no longer consulted for the purpose of making fires, when the savage learned that dry wood will always burn without

their aid. Here the savage has entered into the objective method. "The objective method, by looking *at* things, assuming the position of simple spectator, renounces all hope of ever penetrating the mysteries of existence, of ever knowing the intimate essence of things, and only hopes to detect the invariable order of coexistence and succession." (Lewes.)

To the ancients, who thought the sun a chariot of fire driven across the heavens by a god, our modern ideas of gravity and the relative fixity of the sun would be as incomprehensible as the natural laws, which we know to govern the motions of the planets, are to us. While these laws bring us no nearer to the spirit which regulates the universe, they do have the merit of bringing celestial and terrestrial phenomena into general harmony, and of admitting of a systematic unification of all observed facts. Besides this scientific value, such forms of knowledge remove all traces of superstition, thus making all natural phenomena invariable in their courses. By this method prayers and supplications will no longer be maintained for invoking warm or cold weather. We look to the thermometer and barometer, and watch for areas of high and low pressure, knowing that, under the same conditions, the same phenomena will always occur, in spite of the anger, hatred, or compassion of some god.

The spark of civilization was first kindled in the East, where, at the time of the dawn of history, observed facts were already arranged in what we may call subjective systems. These facts were founded upon experience; man knew at this time how to shield himself from the forces of Nature as well as from the forces of man. He knew how to prepare and cook his food, was physician enough to cure by the use of herbs, biologist enough to distinguish different forms of life, geologist enough to know the lasting properties of different building material. But what is more, language, both written and spoken, was well enough advanced to give us the records of these early times. The fact that one hundred thousand men were forced to work for twenty years upon one pyramid shows that the art of government had advanced no little way.

But through all such knowledge ran pure religious superstition. Nothing existed for aught but the gods; nothing was of use or consequence save that which could apply to religion. Thus narrowed, it was impossible for the conservative minds of the East to

break the strong adherence to custom, which was carefully fostered by government. The people lived on from generation to generation, as their forefathers had done, satisfied with what they had, scarcely dreaming that there could be anything more to learn; indeed, they could not learn more without offending the gods. What Verschoyle says of art in Egypt — that "it was exactly that liberty of spirit that Egypt lacked; art was swathed in bands like a mummy, was forced into the same cold rigidity, and remained immovable like the monuments raised by a despotic empire, under a sky as unchanging as themselves"[1] — may be said with equal truth of all Eastern science.

Among these nations Egypt is the best known. It was governed by a despotic monarchy with divine attributes. The priests had strong influence, strongest after the fourteenth century B.C., but never gained absolute control of supreme power. Mena was the first of historic kings, and the date of his reign varies with different authorities, from 5702 (Boeckh) and 3623 (Bunsen), to 2700 and 2440 (English short chronology). According to Lepsius, the old empire was founded in about 3000 B.C. by Mena; the capital, Memphis, was named after him. The greatest pyramids were built between 2800 and 2700, by the kings who composed what is known as the "pyramid dynasty." About 2221 Amenemhat III constructed Lake Meri (*i.e.*, "lake of inundations"), a large reservoir for regulating the water supply of the Nile; and he built south of this the so-called Labyrinth, a large palace for ceremonial acts and sacrifices. About 2100 Egypt was conquered by the Hyksos, or Shepherd kings. These people were wandering tribes of Semitic descent, who were finally driven out of power by revolts headed by Thebes between 1800 and 1659. From this time the New Empire increased rapidly in power and extent. Under the new kings the Syrians and Ethiopians in the south were conquered; trips were made to Arabia and to the Euphrates. But the "golden era" for Egypt was from 1388 to about 1200, and the greatest name is Ramessu II (Rameses II), during whose reign Egypt was covered with magnificent buildings, and a canal was commenced between the Red Sea and the Nile.

In 730 the Ethiopians under Shabak conquered Egypt and governed for fifty-eight years, when the Assyrians, under Esarhaddon and

[1] J. Verschoyle, "History of Civilization," page 41.

his son Asshurbanipal, put an end to their reign and entrusted the country to twenty governors, most of whom were natives.

One of these governors, Psamethik (Psammethichus), in alliance with Gyges, king of Lydia, with the help of Carians, Phœnicians, and Ionians, made himself independent of the Assyrians, and sole ruler over Egypt, 653 B.C. Out of gratitude to his mercenary troops, he listened to their appeal and opened Egypt to foreigners, who immediately settled in various parts. This led to dissatisfaction among the warrior caste, for the Greek soldiers were favored over every one else. This dissatisfaction was the cause of their migration to Ethiopia, and thus began the decay of the empire.

This ends our short sketch of the history of Egypt; it is needless to go further, for in 525 the ancient country was conquered by the Persians under Cambyses, and with this subjection Egypt, as a nation, loses all historic interest, and her monuments alone show her place in the history of civilization. Science left to the Egyptians would never have gotten out of the ruts which they made for it. New peoples coming in, however, could accept the old ideas for what they were worth, and add to them new ideas and discoveries. Long before the conquest Egypt was known to other nations through the agency of the Phœnicians, the great carrying people of ancient times. To commerce alone do the Phœnicians owe their fame; we hear of them carrying Egyptian products to the East, and, in return, bringing spices and other Oriental goods. In this manner, ideas of Egyptian learning were brought to all neighboring peoples, and these, when the events we have mentioned made it possible, thronged into Egypt to partake of the numerous possessions for which that country was famous. The Assyrians, who were superior to the Egyptians in civilization and knowledge, as the latest discoveries show, had an opportunity to benefit by the Egyptian knowledge as well as to improve the Egyptians themselves; although it is doubtful if the latter would accept new ideas, because of their firmly grounded conservatism. However, the establishment of the Assyrian power in 653 B.C. was a grand opening for foreigners. The Phœnicians and Greek pirates had so filled the Western mind with ideas of the wealth and treasures offered by that vague land of myths, that, when the opportunity came, the Greeks were not slow to accept.

A brief review of the civilization of the East will show what it had to offer to the incoming peoples from the West. From the first, the Egyptians were favored by every force of Nature, and were almost compelled to observe. A remarkably clear sky, never overcast by clouds, and a dry atmosphere which allowed the sense of sight free range to observe the motions of the heavenly bodies, both aided the study of astronomy. The yearly overflow of the Nile, leaving vast fields of fertile mud, made artificial measurements of land absolutely necessary. The overflow of the Nile, again, was the cause of certain hydraulic arrangements for keeping out unnecessary water, if the flood registered too high on the Nilometer, or for the allowance of greater overflow if it registered too low.

At the time of the building of the pyramids, the Egyptians must have made considerable advance in civilization; they knew how to quarry rocks of the hardest stone, even granite, and transport them great distances. They raised huge monolith obelisks, and polished and carved their granite so as to last thousands of years. The Pharaohs divided the land into nomes, and these again into four parts as follows: (1) The chief town, with the seat of government, civil and military, and the center of the local religion; (2) the productive land, bearing the fruits of the Nile valley; (3) the marshes, where the Nile overflows and leaves deep pools not easily dried; (4) the canals, which were filled from the Nile, and used for irrigation when the flood retired. This act of the Pharaohs shows that there was considerable knowledge of geometry and hydraulics; practical geometry was necessary because the Nile overflow of mud obliterated the landmarks, necessitating a yearly measurement of areas; their canals and dams showed a certain acquaintance with practical hydraulics. Chemistry derived its name from Egypt, where it was known as the "sacred art." They seem to have had some chemical knowledge, but it was the result of chance discovery, and not at all scientific. They had no commerce, because their religion kept them from leaving their own country. Arithmetic was unknown to them; they expressed units by strokes, tens and powers of ten by fresh strokes. In medicine everything was specialized, and no physician could undertake to cure any ailment not in his special line. Egypt was a very healthy country, so that new diseases were uncommon. Doctors were required to practice according to certain established precepts

which were sanctioned by the priests. If a patient died under this treatment, nothing was said; but if a death occurred under the application of some new remedy, the penalty was very severe. It was the same in medicine as in every other thing Egyptian; bound down by iron laws, practitioners could learn nothing new, even if they so desired. As for dissecting the dead and learning facts in that manner, they would as soon have thought of burning their sacred bull for amusement.

While all this was good as far as mere knowledge is concerned, it was not science, nor were facts collected by scientific methods, but rather by chance or, perhaps, necessity; use alone sanctioned these facts, not principle.

In astronomy the Egyptians came nearest to modern scientific methods, but here their claims to the origin of the science must give place to those of their neighbors, the Chaldeans.

The first time marked by the Egyptians was the lunar month, and as the overflow of the Nile was the most important event of the year, they were led to divide the period between two successive overflows into twelve months of thirty days each. The inundation of the Nile coincided with the rising of Sirius, the Dog Star; hence the Egyptians, with that tendency of man to mistake coincidences for causes, soon attributed the yearly overflow to celestial agencies, through the rising of Sirius. Coming year after year, this soon came to be anticipated, and the calculation of the number of days elapsing between each rising soon followed; this calculation showed a period of about twelve lunar months, or nearly 360 days. But finding that by this the Nile rose about five days later each year, they soon learned to intercalate five additional days, making in all 365 days. It is uncertain when this first took place; some trace it back only as far as the time of the Shepherd kings, or about 2100 B.C. There is a curious myth told as to the origin of the five intercalary days. Thoth (Hermes), god of astronomy and calculation, plays at dice with the moon, and wins from her a seventieth part of the 360 days of which each year consisted; this fractional part ($\frac{360}{70} = 72$) gives the extra five days which were consecrated to five gods. Still the astronomical year was six hours ahead of this, or one day in four years, 30 days in 120 years, and 365 days in 1,460 years. The Sothiac period, the period between successive heliacal risings of

Sirius, was 1,461 years; so that this extra unit goes to show that they made use of the additional day every four years in their calculations. "The period is called Sothiac, because the time assumed for its commencement was when Sirius, or the Dog Star, called by the Egyptians 'Sothis,' and consecrated to Isis, rose heliacally on the first day of Thoth, the first month of the Egyptian fixed year, the 20th of July of our reckoning." In the Rameseion, an astronomical monument in Egypt, there is a figure of Isis (Sothis) in the middle of a vacant space between the months Mesori and Thoth (last and first). It is inferred from this that the monument was in commemoration of a Sothiac period; the chronology of Egypt is favorable to this inference, as Rameses II or III lived during the fourteenth century B.C., and astronomical reckoning shows that Sirius rose heliacally on the 20th of July at Heliopolis in 1322 B.C. The Egyptians used their astronomy chiefly for astrological purposes; nothing was ever done with it by them, but the facts which they gained were used by the more imaginative Greek.

The Chaldeans were the first astronomers, and their method of astronomical study was infinitely better than that of the Egyptians, who were inclined to watch one star only, Sirius in particular, probably for the sole reason that their ancestors had done so before them. The Chaldeans studied the whole heavens, made a map of the celestial sphere, and were the first to establish the twelve signs of the zodiac. They, also, used their astronomy in religion, and for astrology. They could predict eclipses of the moon with a precision which leaves but little for modern science to improve. They fixed the tropical year within twenty-five seconds of the truth, the sidereal year only two minutes in excess. They also knew of the precession of the equinoxes.

But not only in astronomy were the Assyrians superior to the Egyptians; they had a system of weights and measures based upon a unit of about 500 millimeters, and this was the only scientific standard until the establishment of the metric system. It is a curious fact that these people began with ten and afterwards changed to twelve, probably because the latter number could be more subdivided than the former, which is, indeed, awkward, except for the ease of its multiplication; to the ancient Assyrians, however, this advantage of ten was lost, because the cipher was not then invented.

The Assyrians welded iron and bronze together better than it can be done today; their system of canals, dams, and locks for irrigation was more thorough than ours. They printed by means of revolving rollers, on which were engraved cuneiform characters. Assyria had more to offer to the Greeks than had Egypt; but, while Assyrian knowledge affected Greek learning, still Greek science is due more to the facts learned in Egypt. They did accept Assyrian art; this they could take while marching through a country — that is, they could see it — whereas science had to be studied, and the Greeks never remained in Assyria long enough for this.

The Phœnicians did not have much to offer in the domain of knowledge. Their functions were rather those of a go-between, to carry ideas from one nation to another with the exchange of commodities. They were, however, first-class glaziers and expert miners, and their dyes are famous to this day. To them we owe the origin of the alphabet in Europe, for they communicated letters to the Greeks, the idea being to represent the material object which the name of that object signified.

The aim of this article so far has been to show upon what facts science was based. Eastern peoples amassed details of knowledge, showed great intellectual activity, and made practical use of many arts; but we must seek in the Western mind the inauguration of the true scientific epoch. It was the Greek who threw away the subjective attitude and studied facts for their principles, rejected supernatural agencies, and made a systematic effort to detect the causes of all changes as inherent in the things themselves.

The facts, amassed in the East, furnished the Greeks a groundwork upon which to base their speculations; Thales, for example, who lived about 600 B.C., was the first philosopher to take advantage of the learning of Egypt. There he gained knowledge of practical geometry, which he transformed into the geometry of lines, an abstract subject, by the application of that speculative spirit for which the Greeks were famous. By reasoning, he showed that different parts of a geometrical figure may be obtained when certain other parts are given. This was something new in the history of thought, and at this point we may mark the beginning of that rearrangement of knowledge by which the religio-practical application of facts gave place to the theoretical. It was a long step towards the transition from the subjective to the

objective method of inquiry. Beginning with abstract reasoning of geometry, it spread, later, into all branches of science, seeking the different principles which cause different phenomena. Eastern nations were also speculative. The philosophy of the Hindus far transcended that of the Greeks in imagination, but was useless; while the Greeks were much more practical, and their philosophy was within the range of possibilities. It has been said that an imaginative mind is absolutely necessary for a scientific investigator. It was this mind that the Greeks possessed, although their imagination was a trifle too strong when, through it, they were led to explain the origin of the universe long before they were rid of the local ideas connected with the significance of certain words such as "form" and "matter," "upwards" and "downwards;" and this manner of speculating was the cause of their failure in science. No sooner did they make sure of one or two points, than they must need apply the same thing to analogous phenomena without the test of experience. In other words, they jumped at conclusions, were satisfied with logical explanations, and failed to follow up their conclusions with experiments. In many cases their wild guesses came remarkably near to modern scientific truths. Thus Pythagoras made the sun a glowing ball of fire about which all the planets revolve. Democritus asserted that "nothing can be made from nothing," and that all changes consist in the separation and rearrangement of particles; also that there exist nothing but atoms and empty spaces, and that the atoms affect each other only by impulse and pressure. Anaxagoras maintained that the moon had mountains and valleys, and that there had been grand epochs in the history of the globe. Some of the most striking features of the cult of Zenophanes have been reproduced by Newton.

In their philosophical speculations the Greeks sought after one thing only — an ultimate cause of the universe. In this search many scientific facts were collected as by chance, but the period when science was to be cultivated for practical purposes was not yet come.

Greek thought starts with geocentral and physical ideas. The earth is the grand object of the universe, and, as a necessary result, erroneous views of the relations and dimensions of the sea and air were prevalent. The philosophy of Thales, founder of the Ionian school, is usually summed up in the phrase, "All things are water;" and

to carry out this idea, he attributed all things to this first cause, water, the necessity of which in Egypt may have greatly impressed him. Thales is also universally recognized as the founder of Greek geometry and astronomy. He is said to have predicted an eclipse of the sun which took place May 25, 585 B.C. But this was impossible, because the mathematics necessary for such a prediction were not known until later. His discoveries in geometry, whereby he applied abstract reasoning to practical measurements, may be summarized as follows:

1. The relations of the three angles of a triangle to two right angles.

2. The first trace of the conception of geometrical loci, as shown by his proposition that an angle inscribed in a semicircle is a right angle.

3. The sides of equiangular triangles are proportional.

The philosophical doctrine of Thales was developed, during the course of three generations, by Anaximander, who chose as his principle, not water, but a corporeal element midway between fire and air on the one hand, and earth and water on the other; next by Anaximenes, who preferred air; last by Heraklitus, who chose fire because it was in constant change; if there is to be change at all, he says, it must be constant. All of these philosophers followed the same general plan of ascribing to the universe one ultimate material cause.

Pythagoras of Samos, like Thales, spent much of his time in travel; and after visiting Ionia, Phœnicia, and Egypt, finally settled in Crotona, where he founded the Italian school of philosophy. In his philosophy we find at once an illustration of the speculative tendency of the Greek mind; for this system, more advanced in thought than that of the Ionian school, is much more abstract. The principle or essence of all things is not considered concrete, like water, air, etc., but abstract. "All is number," said the great Samian; and by that he meant not merely that all things can be treated numerically, but that the composition and constitution of everything are determined by number. Virtue, for instance, is a harmony of certain numbers, on the same principle that one is the point, two the line, and three the plane. Pythagoras thus anticipated the modern chemist, who, in like manner, attaches significant numbers to the names of things, who takes hydrogen as unity, gives oxygen 8, sulphur 16, etc. Carrying out these principles, there is no substance whatever to which the chemist could

not give a number. So it was with the Pythagoreans, and they, carrying their numerical doctrine to the extreme, assigned certain numbers as the representation of a bird, a horse, or a man. Pythagoras also taught an emanation theory, for he considered that the source of all numbers is the One or unity, from which he derived his grand standard of harmonical ratio — the musical octave. He discovered the relations of the octave to the third, fifth, etc., by experiments which are used today in every course of lessons in acoustics; but no sooner did he ascertain these ratios than he jumped to the conclusion that he had discovered the world's secret, and so taught that harmony, proportion, and number rule all things. The planets numbered five, which, together with the sun, moon, and earth, are placed apart at distances determined by a musical law. In their rush through space, they give rise to a sound, the "harmony of the spheres," which we do not notice, because we have always heard it. With the Pythagoreans we have the first enunciation of the heliocentric theory of the universe; but they also believed in ghosts and demons, and supposed that souls can exist without the body, leading a sort of dream life. They identified the motes in the sunbeam with these absent souls.

The yearning of Greek minds for explanations, which would combine the present intellectual activity with the old polytheism, found no satisfaction in philosophy, and was equally futile in other branches of learning. Herodotus could not reconcile the inconsistencies of the Trojan War with his knowledge of human nature; Eratosthenes saw contradictions between the voyage of Odysseus and the truths of geography; so Zenophanes, founder of the Eleatic school, gave up the attempt to save the old ideas, and taught disbelief in polytheism and belief in the *One*. This *One* to him was God, who is without passion or motion, and is everywhere — "all eye, all ear, all thought." His followers, Melissos and Zeno, carried on his doctrines which he taught in verse. The latter affirmed his position by the *reductio ad absurdum* — a dialectical method which he was the first to employ. Parmenides, probably a pupil of Zenophanes, would not accept the universal mixture of the One — the Element — and the Many, but declared that these should be studied separately. Division in thought was the direct result; Empedocles and Anaxagoras studied the Many, Melissos and Zeno the One. Both divisions failed, and scepticism came in; suspicions were aroused that the mind of man can obtain

no perfect knowledge, and finally the opinion prevailed that we can have no reliable criterion of truth. The Sophists, or doubters, compared the different theories, showed how absurd each was, and thus all fell together. This period of doubt was the immediate forerunner of more correct opinions as to the nature of the world. In it came the reproduction of the heliocentric theory, with the necessary subordination of the earth, which endangered the old geocentric ideas and belief in the gods, thus giving rise to persecutions, and, in consequence, hypocrisy amongst philosophers. Socrates was the first to raise thought from this position of intellectual anarchy. He saw that physical inquiry was the cause of disbelief; so in his doctrines all mathematics and physics were to be abandoned. In this view he was followed by Plato, who taught the existence of a personal god, and belief in the future as well as in the past life of the soul. While not directly opposed to physical science, he considered it unworthy of much thought. All things are subservient to the intellect, and that alone should be cultivated.

As the first philosophic era was brought to a close by the Sophists, so the later Platonic era was thrown into confusion by the Sceptics, as evidenced by the conclusions of Pyrrho, which amount to saying that, if there can be no criterion of truth, there can be no certain ground of science, and so nothing remains but doubt. But this school of Sceptics never accomplished much, for it was about that time that the teachings of Aristotle began to turn men toward the study of science at Alexandria. It is impossible to pass over this period without some reference to Hippocrates of Cos as a premonition of the coming intellectual display of scientific inquiry at Alexandria. Hippocrates lived about 400 B.C., and to him is attributed the foundation of medicine as a science. The healing of wounds and cure by herbs had been known for centuries as an art, but Hippocrates is the first to attempt to base the practice of his art upon rational and scientific principles. His ancestors were disciples of Æsculapius, and offered prayers and sacrifices to the gods as remedial measures for disease. Philosophy and observation caused him to throw aside all traditions, and led him to declare openly his ways of curing, and to frankly acknowledge his failures. His doctrines ruled the practice of medicine for ages, and his ideas are still prevalent; so that today, when we speak of "bile," or of being "bilious," or of "humors," we use the phraseology of the famous

theory of Hippocrates, that the human body contains four humors, namely, blood, phlegm, yellow bile, and black bile — disease being caused by the undue accumulation of some one of these. This theory led to various remedies, such as bleeding, purging, blisters, and others, some of which are used to this day.

Scientific investigation of observed phenomena began with the Alexandrian school. In the great Museum at Alexandria the amassed facts collected in Eastern countries, were thoroughly examined and rearranged, and from them scientific principles were deduced for the first time. Before this, all had been art, or the realization of principles; as the art of the builder appears in the structure which he has erected, though he may never have thought on the abstract propositions on which its stability and strength depend. On the other hand, science is the theoretical discovery of these principles; it is speculative, not practical. The scientific builder sees that, under certain conditions, bodies must sustain each other's pressure.

It is fitting that Egypt should be the site of the beginnings of science, as it was the site of the beginnings of her parent, art; and, before entering into this scientific epoch, inaugurated at Alexandria, we may pause.

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DISSENSIONS IN THE AMERICAN CHURCHES OVER THE SLAVERY QUESTION; ESPECIALLY AS ILLUSTRATED IN THE PRESBYTERIAN AND METHODIST EPISCOPAL DENOMINATIONS.[1]

By BENTON STURGES, S.B.

The history of slavery agitation in the American churches falls naturally, as does that of political abolition, into two distinct periods. The first, extending from the earliest attacks upon the slave trade by the Society of Friends, down to the years following the Missouri Compromise, is essentially an era of belief in abolition, churchmen and statesmen alike holding anti-slavery opinions almost without exception. But these abolitionists of the "Old School" put trust in schemes of colonization and emancipation through sale of public lands, and, moreover, were many of them Southern men. Even up to 1826 the North could claim scarcely a fourth of the hundred and one anti-slavery societies, while in 1840, out of two thousand, almost all were of the North or West.

The beginning of the second period is marked by a growing aggressiveness on the part of the South. State legislatures passed repressive laws against emancipation and against the education of the slaves, and with Jackson's administration began a general system of vigilance committees, who soon terrorized the Southern abolitionists into silence, or caused their emigration.

The abolitionists, too, became in this period more violent than before, and as a Southern minister said, used measures which, "instead of making an impression upon the hearts of the slaveholders, raised a protuberance." The churches, after carrying their temporizing policy so far as to unsay all their former testimony, ended in disruption, and hastened the division of the country. It was said that Dr.

[1] Thesis, Course IX, 1890.

Palmer, of New Orleans, "did more good in the rebel army for the cause of secession than ten thousand men," and the influence of such a man as James H. Thomwell was incalculable.

First Period, to 1830.

The honor of initiating the anti-slavery movement in America belongs indisputably to the Society of Friends, and the steadfastness with which they maintained and asserted their convictions distinguishes them honorably from the larger denominations.

The subject of slavery seems first to have troubled the consciences of certain Germantown Friends as early as 1688, when they presented a protest at their Monthly Meeting concerning the unlawfulness of buying and keeping negroes, basing their arguments upon the Golden Rule. "There is a saying that we shall doe to all men like as we will be done ourselves. Being now this is not done in the manner we would be done at, therefore we contradict and are against this traffick of men-body." The Monthly Meeting, however, considering it too "weighty" for them to "meddle with," "ye tenor of it being nearly related to ye truth," passed it on to the Quarterly Meeting; whence, for the same reasons, it was transferred to the Yearly Meeting of Philadelphia, and here also failed to procure decided action, "it having so general a relation to many other parts." Thus the first agitation died out for the time being, until 1696, when the Yearly Meeting again took it up, advising Friends "not to encourage the bringing in of any more negroes."

In 1711 the attention of the Yearly Meeting was again called to the subject by the Chester Monthly Meeting. Though the most southerly of all the Philadelphia connection, it was here that the greatest persistence was shown. It even appears that a certain bitterness of feeling must have been aroused even at this early date, since, in 1715 the Chester Friends again appealing, the Yearly Meeting seems to have found it necessary to exhort all Friends "not to reflect on one another, either in public or private, concerning the detaining or keeping of their" (slaves) "servants." The following year the Chester Quarterly Meeting again agitated the question of buying negroes. Their minute, however, met with but cold reception in the Yearly Meeting, which advised "that Friends generally do, as

much as may be, avoid buying such negroes as shall hereafter be brought in, rather than offend any Friends who are against it," adding, "Yet this is only caution and not censure." In 1729 another appeal was made, and after the action upon it had been laid over until 1730, the Yearly Meeting finally disapproved the buying of imported negroes, as well as the importing them, which was already a breach of discipline. In 1735 this "advice" was repeated, and thereafter annually, with the exception of 1740, until 1743.

By this time the members of the Society had everywhere become thoroughly alive to the great evils of the slave trade, and were actively preaching its abolition. As early as 1718, William Burling, of Long Island, had published a tract on the "Unlawfulness of Slavery," followed by others. In 1729 Ralph Sanderford, a "money-getting Quaker merchant" of Philadelphia, circulated his "Mystery of Iniquity," and in 1733 Elihu Coleman issued his "Testimony against that anti-Christian Practice of making Slaves of Men." But greatest of all was Anthony Benezet, the French philanthropist, who, joining the Society of Friends, emigrated to Philadelphia in 1731, and devoted his energies to preaching and writing tracts in behalf of the cause. John Woolman, too, in 1746, began his tour of the South, preaching the "sin of slavery," and that "liberty is the natural right of all men equally." In 1754 he published his "Considerations on the Keeping of Negroes."

The desire to be rid of slavery was unanimous in the Yearly Meeting of 1758, and John Woolman, who was fast becoming a central figure, was appointed one of a committee to visit all slaveholders and urge emancipation. From this time on, the subject received most frequent attention, and after 1767 the subordinate meetings made regular statements concerning the number of slaveholders among them. In 1778 there is record of seven slaveholders having been disowned, and in 1783 the Yearly Meeting was declared free of all connection with the evil.

The history of the movement in the other Yearly Meetings is of a similar character, and in New England slaveholding was made a "disownable offense" even earlier than in Pennsylvania. The first mention of any action is in 1716, when the Monthly Meeting of Dartmouth sent a query to the Quarterly Meeting of Rhode Island, asking "whether it be agreeable to truth for the Friends to purchase

slaves and keep them for a term of life;" and in the same year the Friends of Nantucket made a declaration in the negative. Fifty years later the matter was prosecuted vigorously, and in 1782 the Yearly Meeting reported, "We know not but all the members of this meeting are clear of that iniquitous practice of holding or dealing with mankind as slaves." Two years later a rule was passed making any Friend refusing to emancipate his slaves liable to punishment as a "disorderly walker;" but no such case ever arose.

The New York Yearly Meeting was somewhat slower to move, but it reported no slaveholding members only five years later than the New England Yearly Meeting, 1787.

The first step taken in the Virginia Yearly Meeting was in 1757, when a query was adopted asking, "Are Friends clear of importing or buying negroes to trade?" But no unanimous action could be taken until 1768, when it was declared henceforth a "breach of discipline for any member to purchase a negro or other slave," and in 1784 the Monthly Meetings were authorized to disown any member who should refuse to emancipate his slaves.

Slavery once and forever abolished within the Society, the Friends began earnestly memorializing Congress and the different State legislatures. The first petition to the General Government was from the Friends of the Middle States in 1783, who repeated the action in 1790. On this last occasion they were "indecently assailed, and ridiculed as 'Shaking Quakers.'" In Jefferson's administration, however, they received greater respect, and their petitions were henceforth of regular occurrence. Previously, notable petitions had been presented in 1792, 1794, and 1797.

In the first years of the present century the Friends in the Southern States, though never relapsing into the practice of holding slaves themselves, began to decrease rapidly in numbers. Especially was this true in the Carolinas, where the growing intolerance of the pro-slavery faction led to frequent emigration on the part of the Friends. Numbers of them settled in the White River Valley, Indiana, which in later years became the chief line of the underground railroad, and it was largely due to them that in 1806 Jonathan Jennings, at first ridiculed as the "beardless boy" and "the cold potato," was enabled to advocate with some success the policy of "No Slavery for Indiana." In 1816, at the drawing up of the

State constitution, the petitions of these Friends were again influential upon the right side.

In the North, Friends were everywhere prominent in the foundation of anti-slavery societies, and their action in aiding fugitive slaves to escape, and in many cases even enticing them to do so, called forth the censure of almost all the most prominent advocates of anti-slavery, Washington among them.

About the period of the Missouri Compromise, the general movement appeared first to falter, and though individuals continued to preach abolition, and pamphlets were still issued by the various meetings, Friends partook of the prevalent contagion. The old Abolition Society of Philadelphia itself, in which they had been so prominent, died a lingering death in 1829, having shown symptoms of decay as early as 1807, when its meetings were made triennial instead of yearly.

Among the other denominations in this first era of abolition, none could claim opinions nearly as advanced as the Friends. In none was slaveholding declared a "disownable offense," yet all were distinctly anti-slavery, in different degrees. The Baptists and Methodists were most pronounced. Being drawn from the lower classes, they felt sympathy for the slave class rather than for the masters. The Presbyterians were less strongly anti-slavery, and the Episcopalians probably least of all. As the records are most complete in the case of the Presbyterians, I shall consider them first.

The first official notice taken of the subject of slavery was in 1774, when a representation respecting a mission to Africa was presented to the Synod of New York and Philadelphia, then the highest judicature in the Church, by Rev. Dr. Ezra Stiles and Rev. Samuel Hopkins. In this paper the question of slavery came up, but action on that question was deferred to the next meeting of the Synod. At this meeting, however, nothing was said of it, nor was it referred to afterwards. In fact, until 1787 the subject was left without any formal consideration. Then the Synod took action, and finally expressed its approbation of abolition, urging all Presbyterians to use the most "prudent measures" towards final abolition, but advising preparatory instruction of the slaves rather than immediate emancipation. The following year the General Assembly was first organized, and in 1793 this body reëndorsed the action of 1787 by having

it printed in the extracts of their minutes. They were led to do this by a memorial presented "under cover" by Warner Miller, a Friend. Friend Miller was the same man who, the year before, had presented a petition to Congress upon the subject of slavery, which petition a member from South Carolina had called "mere rant and rhapsody of a meddling fanatic, interlarded with texts of Scripture."

The next important action was that of 1815, chiefly due to a petition from the Synod of Ohio (an intimation of the part the West was soon to play), and to petitions from certain elders. The Assembly now declared that "although in some sections of the country, under certain circumstances, the transfer of slaves may be unavoidable, yet they consider the buying and selling of slaves by way of traffic, and all undue severity in the management of them, as inconsistent with the spirit of the gospel. And they recommend it to the Presbyteries and Sessions under their care to make use of all prudent measures to prevent such shameful and unrighteous conduct." The petitioners were then referred to the former action of 1787, 1793, and 1795.[1]

It was at this Assembly of 1815 that a note was first publicly introduced to Question 142 in the Catechism, to the effect that "man-stealing" is one of the forbidden sins, and that by "man-stealers" are meant all those who "bring off slaves or freemen and sell or buy them." This note was erased in 1816, by the General Assembly, upon an inquiry by the Presbytery of Philadelphia concerning notes in general, but the General Assemblies of 1817 and 1818 both decided in direct opposition to the action of 1816. In 1818 the resolutions were the strongest in their anti-slavery character of any ever voiced by a General Assembly of the Church, before or after.

The report made this year was indeed the strongest utterance of the abolitionists from this time until the final division of the Church, and though it soon became a dead letter, at least in the slave States, and was even declared rescinded by several Synods and Presbyteries, it remained unrepealed by the General Assembly, and was thus the stated law of the Church throughout the whole struggle.

The report, as unanimously adopted, read as follows: "It is manifestly the duty of all Christians who enjoy the light of the present

[1] The action of 1795, in answer to an overture from the Presbytery of Transylvania, Kentucky, was merely a reaffirmation of former sentiments.

day to use their honest, earnest, and unwearied endeavors as speedily as possible to efface this blot on our holy religion, and to obtain the complete abolition of slavery throughout Christendom, and, if possible, throughout the world. The manifest violation or disregard of the injunction here given, in its true spirit and intention, ought to be considered as just grounds for the discipline and censures of the Church, and if it shall happen that a Christian professor in our communion shall sell a slave who is also in communion and good standing with our Church, contrary to his or her will and intention, it ought immediately to claim the particular attention of the proper Church judicature; and unless there be such peculiar circumstances attending as can but seldom happen, it ought to be followed, without delay, by a suspension of the offender from all the privileges of the Church, till he repent and make all reparation in his power to the injured party."[1]

After this stupendous effort, however, the General Assembly seems to have felt satisfied that its duty was done, and it lapsed into the most unaccountable and lengthy silence, taking no action of any kind upon the subject until 1836, when both North and South were ablaze, and the pro-slavery men had become the aggressors. Moreover, it does not appear that the action had any other effect than to arouse the anger of the Southern members at a later date, since the rules set forth were never enforced upon a single slaveholder.

The case was widely different in the Reformed Presbyterian connection. Though scattered in considerable numbers over Vermont, New York, Pennsylvania, and South Carolina, the denomination claims notice, not so much on account of numbers, as from the early date at which its members emancipated all their slaves, and the heroism with which this was accomplished in perhaps the most thoroughgoing slave State in the Union.

They were descendants of the old Scotch Covenanters, and held rigidly to their doctrines. They would not in any way recognize the National Government by holding offices under it, nor would they hold communion with other churches, and in their "Doctrinal Testimony," published in 1807, they declared slaveholding a bar to communion.

Slavery had been agitated for some time in the Northern congrega-

[1] The Church and Slavery. By Albert Barnes.

tions, when, in November, 1800, James McKinney and Samuel B. Wylie, as a committee, visited Rocky Creek, South Carolina, where a Mr. Donnelly had accepted a call and was to be ordained and installed. Before the celebration of the communion the committee stated the decision of the Presbytery at its last meeting, to the effect that slaveholders should not to be admitted to the Lord's table. "Thereupon," Dr. Wylie relates, "the committee were no less surprised than delighted to find with what alacrity those concerned came forward and complied with the decree of the Presbytery. In one day it is believed that in that small community of Reformed Presbyterians not less than *three thousand guineas* were sacrificed on the altar of principle. Only one refused to emancipate his negroes, and his location was beyond the limit of the State of North Carolina." From this time no slaveholder was admitted to this Church, either in the North or South, until 1830, when the denomination was much split up by the New Light Controversy.

After the strong position which John Wesley, the founder of Methodism, had taken upon the question of slavery, it is not remarkable that the early members of that Church in America should have been among the strongest opponents of the system. Wesley had stigmatized slavery as "the sum of all human villainies," and American slavery as "the vilest that ever saw the sun." Francis Asbury and Thomas Coke, the first two American bishops, were as pronounced as Wesley, and Freeborn Garretson ranks with the very first of early abolitionists.

Notwithstanding these influences, however, the first anti-slavery ordinance passed by the Church resulted, in 1779, in a secession of the entire body of Southern preachers, numbering more than half the whole. A Southern Conference was held at Fluvanna, Virginia, while Mr. Asbury held a separate one in Delaware, and, although the objectionable ordinance was withdrawn in 1780, the two bodies did not meet together again until 1784. This ordinance declared slavery to be "contrary to the laws of God, man, and nature, and hurtful to society; contrary to the dictates of conscience and pure religion; and doing that which we would not that others should do to us and ours." It further demanded promises of all itinerant ministers to set free their slaves, if they possessed any.

In 1784 the two divisions came together again, and the Church was first regularly organized. The stringent measures which were

now passed are rather surprising in light of the recent division upon the subject. A rule for gradual emancipation was adopted, which all members must sign within twelve months or withdraw from the Society. Those who refused were to be prohibited from communion and expelled. A note was appended, however, which stated that the rules were "to affect members no further than they were consistent with the laws of the States in which they were residing," thus excepting Virginia, where members were to be allowed two years. Six months later the rules were suspended altogether, and never afterwards introduced.

Emancipation by private individuals did, however, take place to a considerable extent, and Asbury, in his journal, states that in Maryland almost all members had freed their slaves. This was in 1788.

In 1796 new rules were passed, but of a weaker character. It was now forbidden to sell slaves, though purchase was permitted on condition of promises of future emancipation, the Quarterly Meetings to determine at what period. Slaveholders before entering the Church should be spoken to "freely and faithfully" upon the subject of slavery, and an annual address should be drawn up urging gradual emancipation. This was omitted, however, the very next year.

The action henceforward seems to have been all retrograde, and, writing in his journal in 1798, Asbury says: "It appears to me that we can never fully reform the people until we reform the preachers, and that hitherto, except purging the travelling connection, we have been working at the wrong end."

In 1804 even the rule against selling slaves was modified, and in a volume entitled, "Doctrine and Discipline of the Methodist Episcopal Church," published in that year, the following appears: "Any member who sells a slave without his consent shall be expelled *unless for humanity or necessity*, as shall be judged by committee of male members of the Society, and upon purchase the Conference shall decide in how many years the slave shall be declared free;" moreover, Georgia, North Carolina, South Carolina, and Tennessee were declared exempt from the rule. In 1808 all rules relating to private ownership of slaves were stricken out of the Discipline altogether, and the matter of sale or purchase delegated to the Annual Conference. After this the General Conference took no further action for nearly thirty years, and after 1820 even what remained of the General Rule was regarded in the South as a dead letter.

In the Baptist Church, divided as it is into numerous and entirely independent Associations, there is no one source of authoritative and general information. It is therefore only from scattered and exceedingly scanty records that a general view of the position of the denomination at large can be drawn. That Baptists were strongly anti-slavery in the last century is at least certain. Drawn as they were from the lower classes, and led by an almost universally uneducated clergy, it is natural that their sympathies should have been rather with the slaves than with the slaveholders. Thus in 1789 we find a general committee of the Virginia Associations passing resolutions of a highly satisfactory character, and expressing hopes that their "Honorable Legislature" would soon "proclaim the 'Great Jubilee.'"

In Kentucky the crusade was begun in 1787 by Joseph Carman, an elder in the Church, who afterwards became prominent in Ohio as an immediate abolitionist. In 1804 a number of ministers calling themselves the "Friends of Humanity" took such extreme views as to call forth the censure of their Association, at which they withdrew and formed an independent Association. In another part of the State an Association numbering twelve churches actually refused church fellowship to slaveholders. Until the excitement over the Alien and Sedition Acts in 1798, the "Free State" sentiment had prevailed throughout all Kentucky.

Second Period, 1830 to the Civil War.

Many causes combined to effect the change which popular opinion underwent at the close of the first abolition era. These causes were threefold in character, namely, political, commercial, and religious.

The political causes grew out of the Louisiana Purchase and the subsequent rapid development of the West which took place in Jefferson's administration. As the new Territories pressed for admission, the pro-slavery faction, alive to the importance of securing them as slave States, used every endeavor to accomplish this end, and the "Solid South," which had appeared for the first time in 1796, now took definite and permanent form as a great pro-slavery party in the struggle prior to the Missouri Compromise.

The great commercial cause was the increased importance of the cotton trade, and the consequent increased value of the slaves. It was supposed that slavery was absolutely the only system by which the plantations could be cultivated, and that emancipation meant the total ruin of the South. A very singular pro-slavery volume containing essays on the commercial, moral, theological, and ethnological relations of slavery, traces, in an article entitled "Cotton is King," a very curious parallel between the growth of the cotton industry and the growth of abolitionism.[1] It might much more directly be compared with the growth of the pro-slavery tendencies of the people whom cotton supported. In fact, as Mr. William Jay said in commenting upon Daniel Webster's pro-slavery speech: "It is now a matter of cool New England calculation. The cotton interests of Massachusetts call for it, and the gentry of Boston are cheering on their senator in his strange and reckless course." So great was the domination of cotton, that churchmen and statesmen alike feared nothing so much as offending the South; while the latter, realizing her dictatorial position, became more exacting and arrogant every day. The calls of the South Carolina papers for non-intercourse with Northern cities where abolitionism existed were answered by anti-abolition riots in New York and Pennsylvania, where bonfires were made of anti-slavery publications.

The religious causes were resultant upon those already named, and incident upon the great religious revival which spread throughout the country in 1815. Philanthropic societies of all kinds now sprang up, and all that were closely allied with one or another of the religious denominations assumed a pro-slavery character. Foremost among them was the Colonization Society. This had been proposed in 1800, and even then, outside of its benevolent mission, was advocated as a means of removing "persons obnoxious or dangerous to the peace of society." In 1816, when it was founded, its president[2] and nearly all the members of the managing committee were slaveholders, and

[1] Philadelphia was especially excepted in these articles.

[2] Judge Washington, shortly after becoming president, shipped fifty-four slaves to the New Orleans market. Charles Carrol, the second president of the Society and one of the signers of the Declaration of Independence, owned nearly one thousand slaves. James Madison, the third president, left one hundred slaves to his heirs at his death, and Henry Clay, the fourth, has been stigmatized as the man "who has done more for slavery and said more against it than any man in public life." Garrison, in his "Thoughts on Colonization," collected a great accumulation of material to show its pro-slavery character.

though many of its earliest supporters, Dr. Hopkins and Dr. Thomson among them, were inspired with purely philanthropic motives, it received its heartiest endorsement from the pro-slavery interests of the South. Here the authorized agents of the Society solicited contributions on the stated grounds that, by removing the free blacks, the slaves would become much more valuable; and the Southern planters responded with the same alacrity which would be manifested today in any great money-making enterprise. The South, indeed, looked upon colonization as "a drain for the excessive increase," and it has been called "an artificial antidote to nature's cure for slavery."

The churches, especially in the South, were warm in their support, and resolutions were passed again and again, expressing "entire confidence in the benevolent intentions of the American Colonization Society." Contributions for it were regularly taken in almost all the churches.[1]

One of the most significant evidences of the enthrallment of religion to the love of gain was the appearance now of the so-called "Godward side of slavery." Rev. James Smiley had been a man of strong anti-slavery convictions until he became the owner of a number of slaves through a second marriage. A most remarkable change then took place, and at the meeting of the Synod of Mississippi in 1834,[2] in an opening sermon he argued strongly for the righteousness of slavery as justified by the Bible. The sermon was almost universally condemned by the rich planters of the region, and in the following year, when he published his book, "Smiley on Slavery," the Mississippi Synod would give it no countenance, and one of the Natchez book-sellers actually refused to keep it on sale. The sentiment soon changed, however, and this new avenue once open to pro-slavery divines, they soon attained a high rate of speed upon it.

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[1] Maryland passed an act appropriating moneys to the Colonization Society, which stated that, "should the negro refuse to be transported, he should be arrested and *forcibly taken beyond the limits of the State.*" Other States did the same. The victims were not, as an English writer said, "glad to jump out of the frying-pan into the fire," but indignation meetings of free blacks were held throughout the country.

[2] This was not the first appearance of "Bible slavery" in the country, as Dr. Cheever supposed. In 1819 Duff Green had published "The Bible Justifies Slavery," in St. Louis, and in 1823 Rev. Dr. Richard Furman ("that great and good man whose name still sheds a pious fragrance throughout the limits of his acquaintance"), in a communication to the Governor of South Carolina, had defended slavery on Scriptural grounds, while proclaiming the "pernicious tendency of free colored population."

There was not a text in the Bible which could not be turned to account by Southern logic.[1] The desire to secure the authority of one of the ten commandments led to the following: "Thou shalt not covet thy neighbor's man-servant. Then it must be right for him to own him, or it would not be wrong to covet him. Therefore slavery is a divine institution."

Owing to all these causes the hopes of the "moral" abolition party, in 1831, to obtain the support of the religious denominations, met with only the most discouraging results. The church organizations themselves were in advance of public opinion in its downward course, and had, in fact, forced pro-slavery opinions upon their members. It was only after abolitionist excitement had reached an extreme height that the church assemblies would do even so much as recognize it. Individual clergymen did, of course, take most prominent positions in the abolitionist ranks. But those who took the strongest stand were, as a general rule, far from orthodox. Theodore Parker admitted that the testimony of the Bible favored slavery, and made this an occasion for sneering at the idea of divine revelation. Channing had certainly drifted far from his early ideal of Puritanism. Rev. Henry J. Van Dyke asserted that "in this country all the prominent leaders of abolitionism, outside of the ministry, had become avowed infidels; and all our notorious abolition preachers have renounced the great doctrines of grace as they are taught in the standard of the reformed churches."

It was the clergy in the American Anti-Slavery Society who began the troubles leading to division there. In 1833 the question of allowing women to address the meeting was brought up. Mr. George Thompson, addressing the Glasgow Anti-Slavery Society in 1841, spoke of it as follows: "When the abolitionists were few in number, despised, poor, and everywhere spoken against, they kept together like sheep in a storm, or at midnight when the howling of the wolf is heard. The cause went on. The number of abolitionists became greater and greater. Now the Reverend So-and-so joined the Society, after a great many 'ifs' and 'buts,' and then a Reverend Dr. So-and-so joined also, after a great many more. And some of them did not like the forwardness of the women. We seldom like those

[1] That of the North was often little better; for example: "'Ye shall buy of the heathen.' There are now no heathen. Therefore you cannot now buy at all."

who outstrip us in zeal." Thus it was that this growing organization received its death-blow from those who should have been its chief support.

Owing to its long silence upon the subject of slavery, the position of the General Assembly of the Presbyterian Church was somewhat ambiguous at the beginning of the second epoch. By some it was called the "Bulwark of Slavery."[1] By others in the North it was assailed no less than in the South as "A great Anti-Slavery Society." But while the North was to a great extent conservative, and the Garrisonian faction was but small, the South had become overwhelmingly pro-slavery. A letter in the Boston *Centinel* for May 29, 1833, from North Carolina, stated: "The moment that interference with the condition of our slaves is seriously attempted by any considerable party in the non-slaveholding States, that moment the Union is at an end. And I pledge myself that not even an attempt to settle a question growing out of the agitation of slavery would be made on the floor of Congress. The Colonization Society is gaining the confidence of the whole South.[2] I rejoice that the abolitionists are running tilt against it."

It is about this period that the abolitionists in the South made their last attempts in the Synods and Presbyteries of the Church. The Synod of Kentucky debated for two days over an overture declaring slavery to be "a great moral evil and inconsistent with the Word of God." Action was then indefinitely postponed. Next year it was introduced again, and a committee appointed to draw up a plan of gradual emancipation; but no formal action was taken. The report of the Synod in 1835 was strongly pro-slavery, and the effect of this action was felt throughout the Church. The Synods of South Carolina and Virginia had passed resolutions in favor of slavery the year before. In fact, by 1835 Kentucky was the only slave State in which free speech had not perished. Before Jackson's administration lynch law had been seldom, if ever, exercised upon abolitionists, but from this time on it was universal.[3] In the Synod of Virginia, 1835,

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[1] James A. Birney published a pamphlet under that title, Boston, 1843.

[2] He might have added, and losing that of the North, and consequently going into bankruptcy.

[3] This evil was much aggravated by the wild rumors which went through the South subsequently to the Southampton slave insurrection in 1831.

a paper was read on the "Views of Scripture," in which it was asserted that slavery was recognized by Scripture in "precisely the same way as the other domestic relations of life," and, though with some dissent, resolutions were passed condemning the actions of the abolitionists. Dr. Carrol compared the latter to an eagle carrying off a lamb, and, suddenly convinced of injustice, letting it drop.

In the North, especially in the West, the local Presbyteries were beginning to take anti-slavery action. The Presbytery of Chillicothe, Ohio, was foremost in this. In 1829 "Messrs. Gilliland and Crothers were here appointed a committee to prepare a pastoral letter to the churches under our care, on the subject of slavery, and report at the spring meeting of the Presbytery." They prepared two, and both were accepted and printed, 1,800 copies being circulated. They were similar in character, and urged non-communion with slaveholders. In 1833 the same Presbytery adopted another strong paper, a copy of which was sent to the Synod of Mississippi. And it was upon the motion of Dr. Samuel Crothers, of the Chillicothe Presbytery, that in 1835 the Synod of Cincinnati passed a resolution declaring the "holding of slaves for gain a heinous sin and scandal." In 1836 the Michigan Synod declared itself for immediate abolition.

In the General Assembly there had been no action whatsoever. Memorials, which had been submitted in great numbers, were entirely ignored or immediately tabled. In a letter from the stated clerk of the Assembly of 1834, printed in *The Philadelphian* of January 23, 1834, the fact is made clear that an anti-slavery overture, and a bundle of pamphlets for distribution, had been received, but excluded from the House by the Committee of Bills, the pamphlets being used as waste paper. The letter says: "The General Assembly has ever acted in relation to this business in resisting all the violent movements of absolute, universal, and unconditional abolitionists."

After the stormy year of 1835, "Mob Year," there seemed to be a growing presentiment that the General Assembly was to take a decisive anti-slavery stand; several of the Synods took independent action before the Session. In Missouri the Synod passed resolutions condemning the abolitionists. In Illinois, on the contrary, it was resolved "that the practice of slaveholding by a professed Christian is a most heinous and aggravated sin, and ought to subject the perpetrator to the discipline of the Church; and that this resolution,

with the former, recommending use of the proper means for speedy emancipation, shall go into the next General Assembly." This action in Illinois alarmed the newspapers in the South. The Columbia (S. C.) *Herald* urged all Presbyteries "to send full delegations to defeat it." The Charleston *Observer* predicted that "the next General Assembly would be the last to convene under the then organization." It further published advice to the delegates stating "that as soon as the discussion of the morality of slavery should come up they should unanimously retire from the Assembly, and not hold themselves bound by its proceedings."

The General Assembly met May 23, 1836. On the subject of slavery, Mr. Miller made the first report: "Resolved, that it is not expedient for the Assembly to take any further order in relation to this subject." This failed to pass, and a minority report declared "that slavery is a heinous sin, and that it is the duty of every one, and especially of every Christian, to free himself from it, and bring about the emancipation of the slaves in these United States and throughout the world." The whole question was now, however, indefinitely postponed under the following motion of Dr. Hodge: "Inasmuch as the constitution of the Presbyterian Church, in its preliminary and fundamental principles, declares that no judicatory ought to bind the conscience in virtue of their own authority, and as the urgency of the business of the Assembly and the shortness of the time during which they can continue in session render it important to deliberate and decide judiciously on the subject of slavery in its relations to the Church, therefore, Resolved, that the whole subject be indefinitely postponed."

The desire for church division now prevailed widely in the South. An anti-slavery member of the Kentucky Synod, writing July 6, 1836, expressed great fears of a secession there, and a correspondent of the *Southern Religious Telegraph* wrote, "I hope such another Assembly will never meet but once again, and then only with full and delegated powers." The editor commented: "A crisis has come; if there can be no compromise, division must be tried." The Presbytery of Concord, North Carolina, and the Presbytery of South Carolina both declared in favor of division; and the Synods of Virginia and South Carolina expressed similar opinions.

An opportunity now occurred in the Old and New School controversy. In 1837 the Old School at last secured a majority in the General Assembly, and decided to exclude the four most objectionable Synods, viz., the Western Reserve, Utica, Geneva, and Genesee, as it happened, all strongholds of the abolitionists. Rev. Dr. Baxter in advocating their excision openly argued that they would thereby "get rid of abolition."

The New School men immediately held a convention at Auburn, and there laid plans of opposition. In 1839 the commissioners of the four exscinded Synods were at first refused recognition in the General Assembly, and only received it by an over-ruling of the Moderator's decision. After the Session had been adjourned, the Old School remained and organized themselves as a separate church, taking possession of the books and other effects of the Assembly. The matter was now carried into the courts, and the law decided in favor of the New School. This decision, however, was over-ruled, and a compromise finally effected.

The Old School now continued to maintain its policy of conservatism, and after declaring its approbation of the objects of the American Colonization Society and deciding upon a general collection to be taken up for it, did nothing further regarding slavery or the negroes until 1845.

In the New School, too, conservatism continued to rule, but not to the same extent; efforts were at least made, though in this first year's Assembly a speaker was called to order when he attempted to speak of slavery and of the action of 1818.

Notwithstanding their conservatism, ministers were being drawn more and more into the struggle. Conventions of ministers were held in Worcester, Massachusetts, and in Hartford, Connecticut, and though the resolutions were usually of a mild form of abolitionism, with qualified statements, still the discussions bore fruit in the ranks of the younger ministers. This was especially the case among the theological students.

At Lane Seminary, Arthur Tappan set abolitionist debates on foot, in which Theodore D. Weld and Henry B. Stanton, at that time students, took part, and upon the prohibition of such proceedings by the trustees, four fifths of the students left, and camping out until a house could be provided for them, kept up their studies alone. In

1835 they were received at Oberlin, which henceforth became a hot-bed of abolition and a general depot for the underground railroad.

The New School was held back in its desire to become free from connection with slavery by a still greater desire of retaining the churches of the border States, which were now wavering in the balance. Standing in this position, urged to take action by the abolitionists of the North and threatened with secession by the border States if they did, the General Assembly "solemnly" referred the whole matter to the lower judicatories, hoping thus to shift the responsibility. In 1840 the subject was again brought up, but only to be indefinitely postponed.

In 1843 memorials demanding action were again most pressing. Western New York, Ohio, and Illinois made especial efforts to secure it, and an important question, resulting from the decision of 1839, now came up. The Synod of Cincinnati had excluded certain slave-holding preachers from its pulpits, and Dr. Ely argued that such action was outside of its constitutional powers. Mr. Barnes maintained the other side. The decision of 1839 was reaffirmed. There was also much debate on the Scriptural relation of slavery. "If Brother Beecher," said Rev. Dr. Wisner, "had been in the place of good old Abraham, and God had told him to take his only son Isaac and put him to death, Mr. Beecher would look up and say, 'Why, Lord, it is contrary to the great principles of your government for me to take the life of my son; I can't do it.' And so Mr. Beecher would refuse to obey a positive precept because it conflicts with what he thinks is a *great principle*. But if God tells me to lay my wife upon the altar, it shall be done." Nathaniel West, a well-known abolitionist, urged the Assembly to "keep amidships as the best place to avoid those internal revolutions to which they were subject who would go upon extremes." It was finally voted that it was "not for the edification of the Church for the Assembly to take any action on the subject of slavery."

This timid policy called forth more local action and more urgent appeals. The Synod of Genesee declared, "The Church should take action for entire extinction of slavery." It did, however, condemn all disorganizing measures of the abolitionists. The New York *Evangelist* urged upon the Presbyteries the importance of selecting delegates who would "do their duty should the subject of slavery

come up." The Ithaca Presbytery, September 4, 1844, declared that the time had arrived when every church should condemn slaveholding members, declaring at the same time the system to be a "heinous sin." The Montrose Presbytery, November 2, 1844, passed resolutions urging brothers to coöperate in removing the "foul blot."

In the Old School, for several years past, memorials upon slavery had been laid on the table without comment. This action was, in 1844, strongly condemned by the Synod of Indiana, and the following year the General Assembly recognized memorials of three different characters, viz., those demanding amelioration of the condition of the slaves, those asking full discussion of the subject, and others demanding explicit declaration that slavery was a "heinous sin," and demanding exercise of discipline in the case of those practicing it. In answer to the latter, a report was accepted, by a vote of 164 to 12, which declared that "slavery could not be called necessarily a heinous sin without charging the apostles of Christ with conniving at a sin." By way of conciliation the Assembly added that they "would not deny there was evil connected with it." This action practically repealed such former testimony as that of 1818, and remonstrances came in from various quarters. The Presbytery of Beaver (Pennsylvania) declared themselves "aggrieved" at this justification of slavery, and the General Association of ministers in Massachusetts passed a resolution that a letter of remonstrance should be sent to the General Assembly. In 1846 the General Assembly declared that "after the action of 1818 and 1845 the Assembly need take no further action." The following amendment was proposed: "Except to say that the action of 1845 is not intended to deny or rescind the sentiments of the Assembly expressed at different times prior to this date." This was laid upon the table by a vote of 86 to 59.[1]

The New School General Assembly was flooded with memorials in 1846, receiving them from the Synods of Indiana, Ohio, and Cincinnati; from twenty-six Presbyteries and nine General Associations. The action was strongly anti-slavery, and though the Southern members voted in a body, a declaration of Dr. Duffield's was accepted by a vote of 89 *ayes*, 27 *noes*, and 4 *non liquet*. This report

[1] The same sentiment was, later, advanced in the form of a resolution, and carried without division.

declared the system to be "intrinsically unrighteous." "The testimony of the General Assembly from A. D. 1787 to 1818 inclusive has condemned it, and it remains still the recorded testimony, *from which we do not recede.*" It further exhorted all churches to put the evil from them, sympathized with the Southern churches "in their embarrassments," and finally resolved to leave the matter "with the Sessions, Presbyteries, and Synods."

Previous to this resolution the roll had been called for individual opinions, and this had almost exclusive attention for twelve Sessions. The arguments were mostly of a Scriptural character, and were conducted with less excitement than was expected. The majority of the demands were for "no action" or "compromise." General McAfee, of Kentucky, declared, "The body has no shadow of a right to touch this subject in the way of legislation;" and almost all the Southern members concurred in this sentiment.

From this time until the withdrawal of the Southern Synods in 1858, the anti-slavery party gained rapidly in strength in the New School. In the same year, owing to the increased agitation in regard to the Cherokee Mission,[1] the American Missionary Association was formed. Article VIII of the constitution declared: "This Society, in collecting funds, in appointing officers, agents, and missionaries, and in selecting fields of labor and conducting missionary work, will endeavor particularly to discountenance slavery by refusing to receive the known fruits of unrequited labor, or to welcome to its employment those who hold their fellow-beings as slaves."

The Old School continued to maintain its conservative policy. Many memorials were presented in 1845, but no action taken. In 1850 the General Assembly declared that its "previous and repeated declarations are such as to render any action unnecessary." In 1861 the Assembly referred all memorialists to its former testimony, and even in 1863, after withdrawal of the Southern Synods, the reaffirmation of the action of 1818, called for by the Presbytery of Illinois, met with a small opposing minority vote. In other words, the anti-slavery spirit now, in 1863, was not as strong as it had been in 1818, when the action was unanimous.

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[1] For the "Relation of the American Board of Commissioners for Foreign Missions to Slavery," see pamphlet of that title, by Charles K. Whipple, Boston, 1861.

The clergy of the South were headed by Dr. Thomwell, "the Calhoun of the Church," and had decided upon secession during the winter of 1861. Dr. Thomwell's influence extended much outside of the Church, and in November, 1860, he had declared in a sermon that the political secession of the South had become "not only a right but a bounden duty." The Synods of the seceded States now organized as the General Assembly of the Confederate States of America.

In the Methodist Church the subject of slavery was again agitated about 1830–32, when the attacks upon the Colonization Society marked the opening of the more violent struggle. In 1832 the "Doctrine and Discipline" was again published, and the changes made since the edition of 1804 are indicative of the general pro-slavery movement in the Church. The beginning of the new anti-slavery movement was greatly retarded by the action of the bishops, who refused to put motions of an anti-slavery character that were offered in the Annual and Quarterly Meetings. They even went further, and in numerous cases those of the travelling ministry who expressed anti-slavery sentiments were tried and degraded from office.[1]

There was, however, no very public or concerted action until 1835, when a number of the members of the New England and New Hampshire Conferences issued an anti-slavery appeal which created a great deal of excitement.[2] It was immediately answered in a "counter appeal" by Rev. Wilbur Fiske, president of Wesleyan University, and for the succeeding months articles were exchanged regularly in *Zion's Herald*, Rev. Orange Scott appearing as a principal champion of the anti-slavery side.

This controversy greatly stimulated the formation of anti-slavery societies among Methodists, and led to the publication of an anti-slavery paper, *Zion's Watchman*, the first number of which appeared in January, 1836. It also influenced many Conferences to elect anti-slavery delegates to the coming General Conference. This, however, met in Cincinnati with strong pro-slavery sympathies. Almost its

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[1] These were: George Storrs, Samuel Norris, La Roy Sunderland, Lucius C. Matlack, and about twenty others mentioned in "History of American Slavery and Methodism," by Lucius Matlack, Boston, 1848, p. 240.

[2] They had organized as a Methodist Anti-Slavery Society in 1833. The article was written by La Roy Sunderland, afterwards editor of *Zion's Watchman*.

first action was to refuse to publish an address from the Methodist Wesleyan Conference of England, in which the anti-Christian character of slavery was affirmed. Moreover, the Conference censured two of its members, who had attended anti-slavery meetings, by an overwhelming majority. It declared its disapprobation in the most "unqualified sense," and further resolved "that the Conference disclaim any right, wish, or intention to interfere in the civil and political relations between master and slave, in the slaveholding States of this Union." Rev. Mr. Smith, of Virginia, in offering an amendment that the names of the offending brethren should be mentioned in the resolution, said, "Let them be brought forth in all the length and breadth of their damning iniquity." This amendment, however, was lost. Only eleven members voted against the passage of the censure itself, and when Mr. Storrs moved that these names should be entered on the journal, his motion was immediately killed by being laid on the table. During the debates the greatest warmth was shown. After some particularly harsh epithets of Mr. Smith, Rev. O. Scott rose to his feet and demanded if the speaker had any allusion to him. Turning towards him, Mr. Smith said: "I have no more to do with that brother than if he did not exist. I wish to God he were in heaven!"—which is, as the reporter of the New York *Observer* intimated, a clerical expression for wishing him in the other place.

Rev. William Winans, of Mississippi, declared in a speech: "It is important to the interests of slaves that, in view of the question of slavery, there should be Christians who are slaveholders. Christian ministers should be slaveholders, and diffused throughout the Union. Yes, sir; Presbyterians, Baptists, Methodists, should be slaveholders. Yes; there should be members, and deacons, and elders, and bishops too, who are slaveholders." He declared that he himself had become a slaveholder "on principle."

As a result of such action in the General Conference, the local action was very diverse. The New York Annual Conference especially indorsed the pastoral "Advice" which had condemned giving countenance to *Zion's Watchman.* The Georgia Annual Conference resolved that "slavery is not a moral evil," and that the "dignified course pursued by our bishops in suppressing the attempts that have been made by individuals has the approbation and profound respect of this Conference." The Baltimore Conference resolved "that the

General Rule concerning slaves be so construed to make guilt or innocence depend upon attendant cruelty, and not upon the simple fact of purchase or sale." The South Carolina Conference declared that slavery was "not a proper subject for Church action, but exclusively appropriate to civil authority." The Kentucky Annual Conference declared unanimously against interference with the subject, but commended the rectitude and policy of the Colonization Society. In the New Hampshire Conference, on the contrary, resolutions were passed expressing approbation of the acts of its two members[1] who had been censured by the General Conference. The English address which had been refused publication in the General Conference was now answered by a body of ministers, who declared that out of the three thousand travelling ministers in the connection there were probably not three hundred who would admit the holding of slaves to be a sin against God.

In 1837, October 25, a Methodist convention was held in New England, and here the question of bishops' rights was discussed at length. This led to a lengthy controversy between Bishop Hedding and Dr. Fiske on the one side, and Orange Scott and Luther Lee on the other, which was carried on through the papers — six letters by Dr. Fiske appearing in the *Christian Advocate* of New York. The bishops had for a long time been accustomed to declare what business was a proper subject for the attention of Conferences, and a great deal of the anti-slavery agitation now took the form of protestation against their right to do so. In order to quiet this, the New England Conference, June 6, 1838, proposed its "Plan of Pacification," which caused rejoicing throughout the South; it was, in fact, a compromise with every point in favor of the slaveholder, and was frustrated by the action of certain abolitionist Conferences. The Maine Annual Conference was the only Northern body which officially indorsed it.

The year 1839 marks the beginning of individual secessions — first in Michigan, where Rev. W. W. Crane led the movement; others took place at Cleveland, Ohio, and at Munroe in the same State. The abolitionists also gained ground in the General Conference of 1840. Two or three Conferences besides those of New England

[1] George Storrs and Samuel Norris.

elected anti-slavery delegates, and abolitionist petitions were sent from four Conferences and over twenty individuals. The first of the latter was presented by Orange Scott. Great efforts were made at this session to secure the addition of "slaveholding" to the forbidden sins of the Discipline, but when finally put to the vote the motion was lost. The Conference further declared that it had no authority to expound rules of discipline, and "that it is the will of the General Conference that Annual Conferences adhere to the law of the Discipline as it now stands." The most pro-slavery action that was taken was, however, the declaration against the admission of colored testimony against white members. Cases of proscription for anti-slavery opinions were now becoming more and more frequent, as well as the number of individual secessions. The first organized secession took place in Michigan, February, 1841, where, in Wayne County five classes numbering eighty members cut loose, and on May 13 organized as "Wesleyan Methodists." They were soon joined by other small classes of deserters. Another considerable secession took place October 20, 1842, in Western Pennsylvania, under J. P. Betker and others. In November of this year *The Wesleyan* appeared, edited by J. Horton and O. Scott, and on the first page they announced their withdrawal. This was a signal for secession all over the country. Two whole churches seceded with Luther Lee in Providence, and others at Carthagena, and Exeter, New Hampshire. There were secessions at five different points in Ohio, at three in New York, two in Pennsylvania, one in Iowa, and others in many of the New England towns. A convention was held at Utica on the last day of May, 1843, and the Wesleyan Methodist Church organized with about six thousand members. Prohibition of slavery was added to the Articles of Religion, and the powers of bishops were much limited.

Owing to the frequency of secession the pro-slavery faction felt the need of strengthening their position. Dr. Fiske, of Wesleyan University, had been elected a bishop, but had resigned; in electing a successor the South urged the addition of one or two more. The North, however, had the majority, and decided it inexpedient to elect any at all.

Another grievance of the South was the decision in the case of Rev. F. A. Harding. The Baltimore Conference, in which he was a

minister, had declared him suspended from the ministry[1] until he should manumit certain slaves which he had acquired by marriage. This he protested he could not do, in virtue of the laws of the State, and appealed to the General Conference, where the decision was sustained by a vote of 117 to 56.[2] In the course of the proceedings Bishop Andrew announced that he also was possessed of slaves, and a preamble and resolution were offered by A. Griffith and J. A. Davis "that he desist from all exercise of his office so long as the impediment exist." The other bishops — Soule, Hedding, Waugh, and Morris — were unanimous against this action, but a motion "that this be considered advisory only" was laid on the table and the resolution carried. The Southern members presented a declaration, on the next day, that further union was inconsistent with success of the ministry in the South, and a committee was instructed to draw up a plan of friendly division. This was presented on June 3, but laid on the table. Hopes were still entertained of preventing division, and the decision in Bishop Andrew's case was set at naught by Bishop Soule, who publicly invited the former to preside at least at one Conference. These conciliatory measures, however, came too late, and a convention of the Southern delegates was called at Louisville, May 1, 1845. This body organized the Annual Conferences represented into the "Methodist Episcopal Church South," to hold its first General Conference at Petersburg, Virginia, May 1, 1846.

The greatest bitterness now arose between the two sections on account of the refusal of the North to give up any part of the Book Concern and Chartered Funds as was provided by the Conference of 1844.

As in the Presbyterian churches after the split into New and Old Schools, the Northern division continued its conservative policy in hopes of attracting the border churches, but with even less success. There was a slight attempt, in 1856, to add the sin of slaveholding to those prohibited in the Discipline, but this failed, while in the Church South the laws against buying and selling slaves which up to this time had been existing, were immediately expunged.

In the Baptist denomination agitation took place chiefly in the

[1] May 18. [2] May 22.

missionary societies. The English Baptists had sent letters to the Baptist General Convention, which refused to publish them. Another was unrecognized by the Board, but answered by an individual member, saying that the Board would not intermeddle. English abolitionists, coming over here with anti-slavery intentions, were led to refrain from public speaking for fear of alienating the whole Church.[1] In 1841 Rev. Jonathan Davis, a Georgia Baptist visiting Boston, boasted publicly from the pulpit that he owned thirty slaves, and would "wade knee deep in blood to defend his right to hold them." He wrote home that but very few of the one hundred and fifty-six Baptist clergymen in and around Boston were abolitionists. Finally in 1840[2] a convention was held in New York "for the purpose of considering the connection of the denomination with slavery and inquiring what could be done." The result was the formation of the National Baptist Anti-Slavery Convention. The Triennial Convention of 1841 aggravated matters by a pro-slavery compromise, and in 1843 many members withdrew from the missionary societies and organized the American Baptist Free Missionary Society at Boston. There was strong agitation now in the old boards to refuse contributions from slaveholders and to cease the appointment of them as missionaries. In 1844 the Alabama State Convention demanded "distinct and explicit avowal that slaveholders are eligible and entitled equally with non-slaveholders to all the privileges and immunities of the Society." Receiving a reply in the negative, they withdrew and organized at Augusta, Georgia, as the Southern Baptist Convention, electing a Board of Foreign Missions with its seat at Richmond.[3]

The Protestant Episcopal Church was probably fully as pro-slavery in spirit as the other denominations, but by careful avoidance of the subject in the discussions at the General Convention, they were able to maintain church union until the political secession had taken place, when a meeting of the two divisions became no longer practicable. That the pro-slavery spirit was strong is clearly evinced by this very silence; for, otherwise, the well-known fact that clergy and even bishops in the South were slaveholders could not have been overlooked.

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[1] 1836. [2] April 28. [3] May 8, 1845.

Their theological seminaries were carefully closed against colored students, the first case of rejection being that of Mr. De Grasse in 1836, who had petitioned the General Theological Seminary. Three years later Bishop Onderdonk recommended a colored student, Alexander Crummel, and he also was refused admittance by the board of trustees. The bishop's recommendation was coupled with the clause, "if you shall think it right and proper to admit a colored member." Colored clergymen were, moreover, excluded from the conventions in New York and Pennsylvania at least, and the latter diocese had an express canon to that effect.

This same spirit is further evidenced by the voice of the Church papers. *The Churchman*, which has always been the most authoritative Episcopal sheet, denied the "right of slaves to read the Scriptures," in set terms. The *Spirit of Missions* gave much room to similar sentiments. Seven pages were given, in one instance, to a scheme of Mr. John McDonough, who, by working his slaves four extra hours each day, was enabled at the end of *only fifteen years* to set them at liberty, finding himself so much the gainer at the expiration of that period that he could purchase twice as many more. The Bishop of Georgia was so delighted with this scheme that he immediately proposed the establishment of a mission school to be placed under the charge of Bishop Polk, and the first expense to be met by gains from a large plantation run on the above benevolent principles. This school was to be named the McDonough Institute, after the philanthropist who had made the great discovery.

Such opinions as the foregoing were so prevalent that the harmony in the Church was undisturbed even by the capture of John Brown, which took place during session of the General Convention of 1859. "The good feeling between churchmen of the North and South occasioned mutual rejoicing."

For a year the condition of the Church was practically the same, the only change being that seven Southern dioceses no longer prayed for the President and Congress.[1] Not until 1862 did the Church divide, and then, as a matter of course, eleven States sent no delegates, these Southern members meeting in Georgia, as the "First Regular Council of the Protestant Episcopal Church in the

[1] It was for this reason that, at a later date, General Butler sent three Episcopal clergymen North as prisoners of war.

Confederate States." The following is extracted from their pastoral letter: "The Constitution of the Protestant Episcopal Church in the Confederate States, under which we have been exercising our legislative functions, is the same as that of the Church from which we have been providentially separated, save that we have introduced into it a germ of expansion which was wanting in the old constitution. The time has come when the Church should press more urgently than she has hitherto done upon her laity the solemn fact that the slaves of the South are not merely so much property, but are a sacred trust committed to us as a people to be prepared for the work which God may have for them to do in the future; to give back to them religious and moral instruction," etc. "It is also our obligation not to violate the sacred relations which God has created, and which man cannot consistently with church duty annul. We rejoice to be enabled to say that the public sentiment is rapidly becoming sound upon this subject, and that the Legislatures of several of the Confederate States have already taken steps towards their consummation. Hitherto we have been hindered by the pressure of abolitionism; now that we have thrown off from us that hateful and infidel pestilence, we should prove to the world that we are faithful to our trust."

As to the smaller sects and independent congregations which have from time to time, and so numerously, cut loose from the larger denominations, the records are, for the most part, inaccessible. It is reasonable to suppose their histories miniatures of those outlined in the foregoing pages. I subjoin extracts from the minutes of the General Convention of the "Christian Church," held at Marion, New York, October, 1850:

"*Resolved*, that we consider the personal freedom of every human being to be a sacred right of his nature; and that slavery, therefore, in this country and in every other, is a great infraction of the right of humanity; that it is not a natural relation; and that we entertain the belief and cherish the hope that the tendency of improvement, inseparable from the nature of man, and the teachings and spirit of the Christian religion, with the beneficent workings of the great Providence which tenderly cares for the welfare of each and all of God's moral offsprings, will finally abolish this relation from the face of the earth.

"*Resolved*, that to remove the evils of slavery it should be approached with no partisan motive; that the love of humanity, rendered wise by consciousness of the past imperfect state of all human society, should temper every word we write or utter on this subject; that we consider no part of the Union as being justified in boasting, regarding the fraternal ties which unite the different parts of our country together, an exclusiveness from sin over another; and that, forever, we ought to treat with kindness and esteem the good men and women of the South whose birth and education have providentially cast them in a region of country inheriting the system of slavery."

In a general survey of the period under consideration, one is struck by the similarity of the results brought about in the religious denominations and political parties by slavery, the fundamental cause underlying the great social revolutions of the times.

After the confusion of the Revolutionary War had subsided, and comparative order had taken its place, political parties became defined upon broad party principles, and their growth was one of steady individuation. In the same way the religious denominations gained a national organization, and each settled into its social and doctrinal rut. These lines of demarcation were, however, soon overshadowed by the unparalleled growth of slavery, which resulted first in the separation in the churches, then in the political disintegration of parties, and finally in the division of the Union itself.

It is astounding that an institution so evidently opposed to all ideas of moral justice should have found its warmest and staunchest supporters in the ranks of prominent churchmen. Such being the case, it is perhaps not so surprising that complete separations should have taken place in the churches sooner than in the political parties; that the moral convictions of anti-slavery ministers should have first rebelled against the compromises prompted by the ideas of political expediency prevalent among party leaders.

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THE PURIFICATION OF WATER BY METALLIC IRON.[1]

By EASTON DEVONSHIRE, Assoc. M. Inst. C. E.

Were any special proof needed to show how rapidly this country is arriving at the age of maturity, it might be found in the increased attention which is given, not only by scientific men and professional sanitarians, but by the people at large, to questions affecting public health. Of these questions, not the least important is that of the quality of the water we are to drink, and the precautions that should be taken to prevent this necessary of life from being a possible source of danger to the health of the consumer. In the phenomenally rapid growth of cities in this great country, sufficient attention has not been paid, in the first instance, to sanitary considerations, which have almost necessarily been postponed for want of time to give them the prominence they deserve. Again, the increase of population and the growth of industries of all kinds in the neighborhood of the waterways, which were the cause of their location, have altered the conditions which originally permitted, if they did not justify, the utilization of natural waters for the drinking supply of cities and towns. In the older countries of Europe, long since densely populated, it has for years past been found necessary to endeavor to counteract by artificial means the inevitable pollution of sources of water supply at one time deemed harmless. Also, the advancement of science has proved the dangers to public health which impure water offers, and which were, until comparatively recent times, but imperfectly recognized.

The oldest method of artificially improving impure water is that of filtration through sand. It is the general practice in England

[1] A lecture delivered before the Society of Arts, Massachusetts Institute of Technology, December 11, 1890.

and in many parts of the continent of Europe to submit water drawn from rivers, or other sources not entirely free from suspicion of contamination, to the process of sand filtration.

This process consists of allowing the water to percolate through a layer of sand two or more feet in thickness at a speed sufficiently slow to remove at least a considerable portion of the suspended impurities. The action of the sand filters, worked as they are in ordinary practice, is purely mechanical, the dissolved organic matter not being reduced in any important proportion. They are, however, capable, as has been shown by Piefke, of arresting bacteria to an extent which their porosity would not seem to render possible, the explanation being found in the formation, after several days' working, of a sticky film or membrane, which Piefke terms "bacteria jelly," to which the micro-organisms adhere when the speed at which the water passes through the sand bed is maintained at a slow and unvarying rate. This speed will differ according to the nature of the water and the standard of improvement aimed at. Under favorable conditions it should not exceed sixty gallons per square foot per day, while half this speed will fail to remove the color due to finely divided clay or peat so frequently met with in river and impounded surface waters.

In 1879, when a charter for the water supply of the city of Antwerp, in Belgium, was granted by the municipal authority to an English firm, permission was given to use the river Nethe as the source of supply, on condition that its water should be submitted to a careful filtration through sand before distribution.

Before commencing to construct the works, however, the proprietors foresaw that simple sand filtration would fail to remove the peaty color from the water and render it sufficiently attractive to the eye to compete with the old established supply obtained from the shallow wells with which each household was provided. It was recognized that whatever superiority the sand-filtered river water might be shown to possess over the contaminated well water, the commercial success of the undertaking would be endangered by the inferior appearance of the new supply. Several chemically acting filtering materials were known to be capable of removing the objectionable color, and, after an examination of the merits of each of

them, the material known as Bischof's spongy iron was selected. Successful experiments were made, and filters containing spongy iron were designed and constructed.

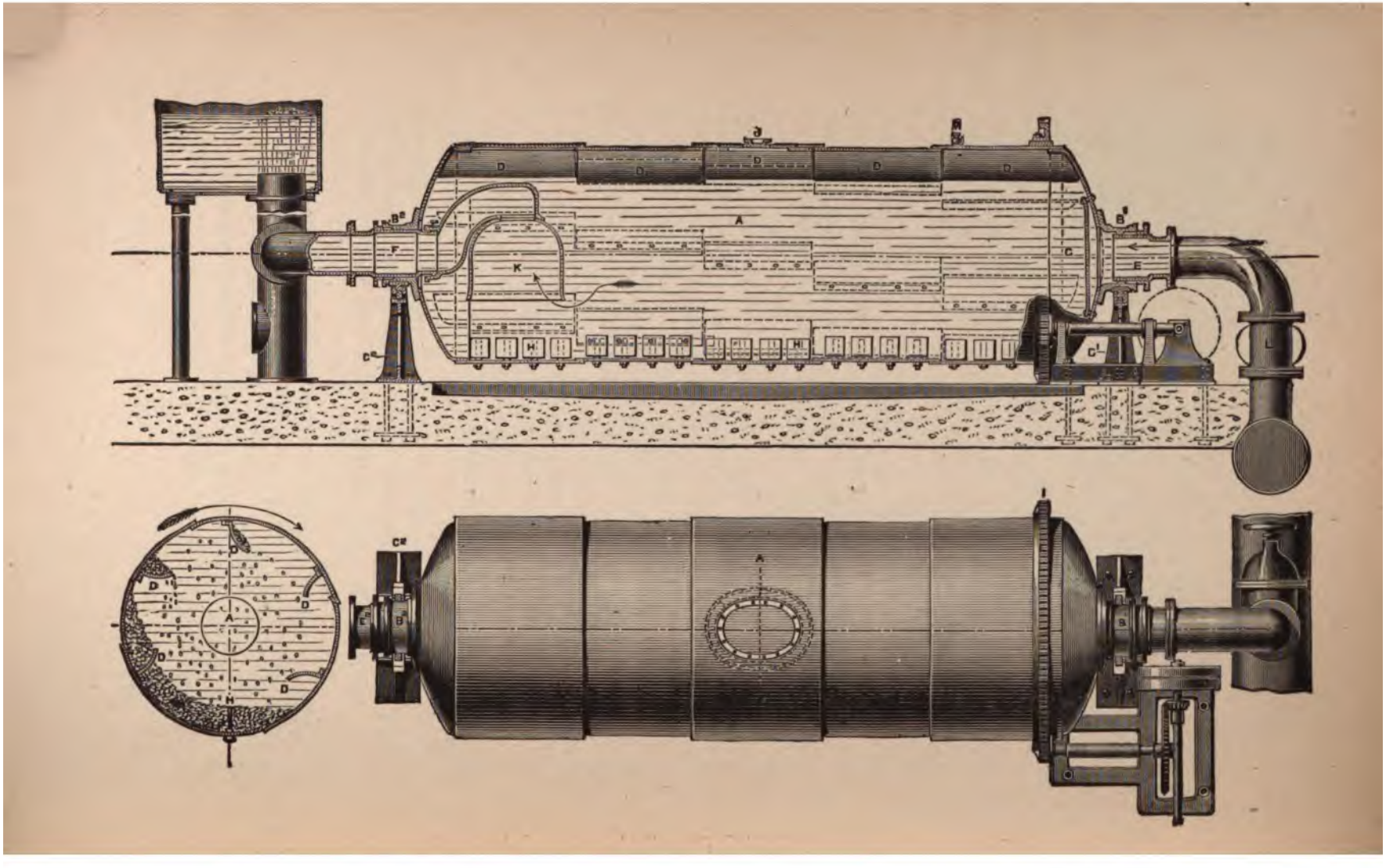
Spongy iron is obtained by roasting hematite ore to a point just short of fusion, the metal being rendered porous or spongy by this operation. Some nine hundred tons of this form of iron, mixed with three times its bulk of fine gravel, were used to form three filter beds. The river water percolating through these beds took up a portion of the metal, forming a ferrous salt, which by subsequent contact with the air was precipitated as ferric oxide, and acting as a coagulant, allowed the water to be strained through a bed of sand, and to issue bright and colorless.

The desired improvement in appearance was thus obtained, and at the same time the water was found to be purified to a remarkable degree. The Bischof process, though costly, thus promised to solve the problem how to utilize an impure natural water without danger to public health.

For nearly three years the Bischof filters realized expectations; but gradually, as the demands of the city increased, it was noticed that the daily delivery of the filters was decreasing in quantity. Investigation showed that the material was caking and rusting together, and was being clogged with the substance formed by its chemical action on the dissolved organic matter of the river water. Finally, when great expense had been incurred in efforts to maintain the porosity of the filters, William Anderson, the consulting engineer of the works, designed a method of overcoming the mechanical difficulties of filtration. This method, known as the revolving purifier system of purification, is the invention which my paper is to treat of tonight.

The somewhat long introduction I have given it is for the purpose of showing what was the necessity for the invention, and what were the difficulties in water purification and filtration it had to overcome.

In Bischof's, as in all other systems of chemically acting filters, the water is brought into contact with the purifying medium by percolating through it. Anderson reversed the principle by designing an apparatus in which the purifying medium is continuously showered down through a flowing stream of water. The apparatus by which the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result is effected consists of a cylinder A, supported in a horizontal position by hollow trunnions B_1 B_2 revolving in pedestal bearings C_1 C_2, placed one at each of its extremities. Within the cylinder are a series of short curved shelves D D D, arranged in steps at equal distances round its circumference, and reaching from end to end. In the place of a sixth row of curved shelves is a line of small square plates H H H, which, by means of nuts outside the cylinder, can be set at an angle with the axis of the purifier. These plates serve to direct the shower of iron back towards the inlet end of the apparatus, and so to counteract the tendency of the current of water to carry the purifying material forward. Through the hollow trunnions, which are provided with stuffing boxes, pass the ends of inlet and outlet pipes E and F, which bring and which carry off the water to be purified. Facing the inlet pipe E is a circular distributing plate G, secured at a distance of ⅜ inch or ¾ inch from the end of the cylinder, serving to distribute the water radially on its entering the cylinder, and prevent its flowing into a direct current along the axis of the apparatus. The inner end of the outlet pipe is fitted with an inverted bell-mouth K, carried down as near to the bottom of the cylinder as the shelves will allow, and intended to prevent the finer particles of iron from being carried out by the current of water. A rotary motion is given to the cylinder through gearing working into an annular spur-wheel I, surrounding one end of the cylinder.

Through a suitable hand or manhole J, iron, in a moderately fine state of division, is introduced into the cylinder and spread evenly along the bottom in sufficient quantity to occupy one tenth of the capacity of the cylinder.

When the revolving purifier is to be set in motion, the sluice-cock L, provided at the inlet, is opened and the cylinder filled with water, the air being expelled through the air-cock M, which at that time must be at the top of the cylinder. When all the air is expelled the air-cock is shut, and the apparatus is set in motion by a small steam-engine, or other suitable form of motor, the speed of rotation being about six feet per minute at the periphery. By this rotary movement the shelves are successively caused to scoop up the iron granules, which at starting are spread evenly along the bottom of the cylinder, and to shower them down through the water.

By means of the sluice-cock L, the flow of water may be regulated to the speed required to give the duration of contact with the iron found necessary for efficient purification under the particular circumstances.

In his first experiments Anderson used spongy iron, which was up to that time supposed to possess special merit as an active purifier. Experiment, however, showed that scrap iron, in any convenient form, was equally active. The great difference in cost of the two materials of course made this discovery a valuable one.

The objections to the practical working of spongy iron filters on a large scale were twofold. First, the rusting together of the mass; and secondly, the coating of the material by the products of its own chemical activity, whereby the contact between the metal and the water was continually being impaired.

The revolving purifier obviates both of these objections; in the first instance by showering a small quantity of the material loosely through the water, and in the second by the automatic cleaning which each particle of the metal gets by rubbing against its neighbors.

The theory of the process is the formation, in the water passing through the purifier, of a protosalt of iron, through the solution of a minute portion of the metal by the carbonic acid and oxygen contained in the water. On issuing from the cylinder into the open air the protosalt is converted by further oxidation into the insoluble form of ferric oxide. In its formation the ferric oxide entangles the suspended matter and oxidizes the organic ingredients of the water. The destructive action of the ferric oxide on the organic matter has been described as being similar to that of rust stains or iron-mold on linen, the fibres of which are apparently burnt up.

Micro-organisms are at the same time caught and held in the precipitate. Dr. Edward Frankland goes farther, and considers that they are actually destroyed, the iron having a toxical effect. Whatever may be the true facts of the case, it has been proved by repeated analysis that the purification of the water is effected by the time the precipitate is completely formed. Subsequent sand filtration has only to remove the precipitate by mechanical straining.

Professor Kemna, of Antwerp, who has studied this process very fully, has expressed the opinion that the micro-organisms are to a great extent arrested by the film of oxide formed on the surface of the sand bed. I, at one time, expressed the same view; but recent

experiments lead me to think that, as far as the bacteria originally present in the water are concerned, the precipitate alone is capable of holding them. Of course in practical working the purified water receives continually a fresh supply of bacteria from the air. These are certainly removed during filtration; and here the coating of oxide, which the sand receives, is of great value, replacing the gelatinous film and the membrane formed by the growth of algæ on ordinary sand filters, as explained by Piefke in his experiments with the Berlin filters. It is also possible that the ferric oxide film on the sand removes the last trace of free ammonia, the entire absence of which is characteristic of the iron process.

Weekly analyses of the Antwerp water before and after filtration show that, in actual practice on a large scale, the albuminoid ammonia is reduced to one third of its original amount; free ammonia and nitrous acid entirely disappear, and nitrates are brought down to the smallest trace.

As regards micro-organisms, continued examinations by Koch's gelatine test show that, from the number of 50,000 to 100,000 in the river water at Antwerp, the highest figure during the whole of last year in the filtered water was twenty colonies per cubic centimetre, the average being only four or five colonies. This means practically the sterilization of the water. The figures are the more remarkable as they are the results obtained daily, under all conditions of the water, and in the fifth year of the uninterrupted working of the process.

The principle followed out in this process differs essentially, in one important respect, from that of the high pressure system of filtration which, in conjunction with the use of alum as a coagulant, has been the subject of many patents in this country.

With high pressure filters the coagulated impurities are strained out of the water while the latter is forced, at a high rate of speed, through a compressed bed of sand of comparatively small area. The effect of the high rate of speed is to impregnate the whole depth of the filtering materials with the coagulated impurities. At a given moment the filter is clogged up through its whole depth, and must be washed throughout by a reverse current before being used again. In Anderson's process care is taken so to regulate the speed of filtration that the sand bed shall be capable of retaining

at its surface the precipitate formed by the iron. In this way, as long as the filter maintains a specified delivery under a constant and low pressure, the certainty exists that the mass of the sand is absolutely unfouled, and water is obtained of the same degree of purity from first to last. Further, as a matter of economy slow-running filters have proved the best. Although of much greater area, the fact that they require cleaning only once in six weeks or two months, as against the daily washing given the high pressure filters, makes their working cost the lower of the two.

When the difference of cost of scrap iron and alum is taken into account, comparative statements of the working cost of the two processes show that for similar waters the iron process does not cost more than half the alum process.

From what may be called the sentimental point of view, the avoidance of the use of chemicals, whether alum or salts of iron, such as perchloride or sulphate of iron, is a strong argument in favor of the Anderson process for the treatment of potable waters.

The actual working cost of the revolving purifier process varies with the nature of the water treated, and may be considered as being more or less in proportion to the activity of the particular water in contact with the metal, and the readiness with which the precipitate forms. A clear water, free from mud and suspended impurities, is by no means necessarily the cheapest purified; for with waters like that of the Mississippi, containing an immense quantity of finely suspended clay as well as much easily separated mud, a few minutes' agitating with plate punchings produces such a coarse and voluminous precipitate that ninety per cent of it can be arrested by obstacles placed in the channel along which it runs on its way to the filter bed. On the other hand, a large quantity of organic matter of certain kinds will retard the precipitation of the iron, and keep the precipitate in a finely divided condition until the sand is reached. It is the finely divided matter, be it a precipitate of ferric oxide or suspended clay, which shortens the life of the filter. Thus, to go to an extreme case, clean sand carried mechanically in water can be strained out with the greatest facility without choking the filter. With the iron process, where a voluminous coarse precipitate is formed, the whole bulk of it may be sent on to a sand filter, and yet the life of the filter will be three or four times as long as with a fine precipitate of small volume.

The workings of the process at Antwerp and Dordrecht, in Holland, serve as excellent comparative illustrations of this. At Antwerp the water is charged with much organic matter, both vegetable and animal, but contains comparatively little finely suspended mud. Before the introduction of settling reservoirs the filters had to be scraped on the average once a fortnight. At Dordrecht, where the water is organically much purer, but holds a great deal of fine clay in suspension, the filters run for five weeks, although the water goes direct on to them from the purifier.

I stated before the Franklin Institute, some eight months ago, that for quantities of five million gallons per day and upwards the working cost of the process in no case would exceed two dollars per million. I have since received the figures of cost for the year 1889. Although on the average less than 750,000 gallons were pumped daily, this being under half the capacity of the plant, the 234 million gallons pumped in the year cost only $257, a fraction over one dollar per million. Of course with large volumes the cost per million decreases.

ONE MONTH'S WORK ON TWO PLANE-TABLES.

By ROBERT H. RICHARDS, Professor of Mining and Metallurgy, Massachusetts Institute of Technology.

The Summer School of Mines, which occurs on alternate years with that of Metallurgy, was held at Ishpeming, Michigan, in June, 1890. One of the chief features of these schools is the geological survey, which is so laid out as not only to give practice in surveying and in drawing geological sections, but also in the use of geological maps and sections as a means of predicting the whereabouts of new ore deposits.

The map here given was prepared from the surveys of the members of the Summer School of Mines, and has served in its execution to show how data must be collected, and how put together in order to be made available as a guide for the bounding of old and the search for new deposits.

A similar map made by the Summer School of 1888 has led to the discovery of a promising ore deposit previously unknown to the company.

The legend written upon the side of the map will be found to explain the main features and facts which the map brings to light.

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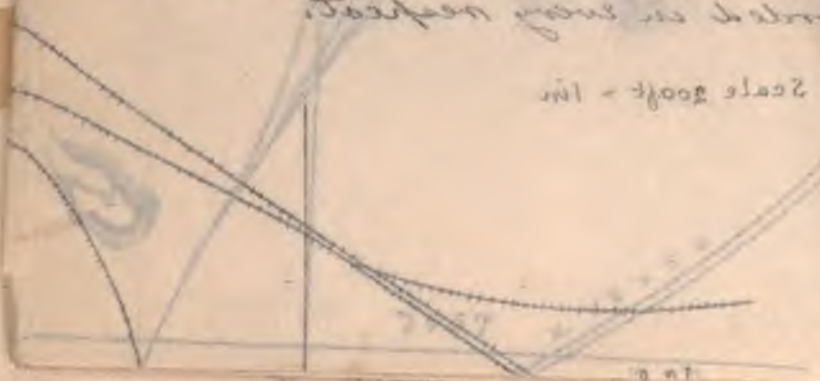

TECHNOLOGY QUARTERLY.

VOL. IV. JULY, 1891. No. 2.

THE ADMISSION EXAMINATION IN ENGLISH.

BY G. R. CARPENTER, ASSOCIATE PROFESSOR OF ENGLISH.

ABOUT ten years ago all, or nearly all, the New England colleges agreed to model their admission examinations in English after a fixed type. At each college the examination was to consist of two parts — the writing of a short composition on a subject taken from one of a given list of books, and the correction of certain specimens of bad English. For several years the Massachusetts Institute of Technology has been following, to some degree, the plan adopted by these colleges. In the catalogue for this year, indeed, it so phrases the requirements for admission in English that they actually coincide in all essential particulars with those prescribed for entrance into the best New England colleges. "The candidate will be required to write," says the catalogue, "a short English composition — correct in spelling, punctuation, grammar, idiom, and division into paragraphs, and plain and natural in style — on some subject already familiar to him. He will be judged by how well he writes, not by how much he writes. For convenience, and in order that the candidate may be acquainted with good models of style, the subjects will be taken from one or more" of certain standard books, with all of which the applicant is expected to be familiar. "The candidate will also be required to correct specimens of bad English set for him at the time of the examination." [1]

[1] See Professor L. B. R. Briggs's article on the Harvard Admission Examination, in *The Academy* (Syracuse, N. Y.) for September, 1888, and Professor A. S. Hill's chapter on English in the Schools, in "Our English" (Harper & Brothers, 1889). Strang's "Errors in English Accidence" (D. C. Heath & Co., 1888) and David Salmon's "Senior School Composition" (Longmans, Green & Co., 1891) indicate the kinds of books best suited for preparatory school work.

Now that the Institute has virtually accepted the same standards and the same requirements that are in vogue in the best New England colleges, we should do well to see clearly what principles underlie these requirements. Briefly, these principles, it seems to me, are two: first, that a boy of sixteen or seventeen, properly fitted for college or for a technical school, should be able to write correctly and with some ease on subjects which he already knows; second, that he should know what is right and what is wrong in matters of grammar, syntax, and the like, about which there can be no possible doubt. These two principles, it will be noticed, are respectively the bases of the two parts of the admission examination.

1. The first point is one which our preparatory schools sometimes wilfully, sometimes carelessly, neglect. Were it possible, most teachers of English would, I think, prefer that there should be no special admission examination in English, and that the candidate should be judged by the English of all his examination papers, not by the English of a single paper. Such a system is not at present practicable at any school or college which I know; but we should bear in mind that the object of this part of our examination in English is not to satisfy ourselves that the candidate possesses a given amount of information, but simply to make sure that he is sufficiently fluent and correct in expression to enable him to carry on his work in other branches, as well as in English, with profit to himself and without hindering others. For in college and in technical school alike the general complaint of the various departments of study is that many students are unable to express themselves satisfactorily. Such students come to us spelling badly, without any proper sense of English idiom, writing awkwardly and with great effort. They are obviously, in short, as unfitted for many parts of their college or professional training as they are for that written communication with private individuals or public bodies which, to a greater or less degree, must form a constant part of every man's professional or private life. Such men, then, come to us heavily handicapped in our common struggle for expression; and it is to the preparatory schools that we look for such methods and results of teaching as shall reduce to a minimum this percentage — already too large — of students who may perhaps be able to think, but have no adequate means of showing us by written words that they think or what they think.

As a matter of fact, the bulk of our preparatory schools are doing

nothing of the kind. In the classics, in modern languages, in history, in mathematics, in physics, there is, I understand, a marked endeavor on the part of preparatory schools to train the candidates for colleges and technical schools by methods which the higher institutions themselves approve. In English alone the fit which the preparatory schools give is too often not a "fit" at all, but a mere guess at the easiest way to fulfill the letter, and not the spirit, of the requirements. This state of things, it seems to me, is due to a general failure on the part both of preparatory schools and colleges to agree on the position which the study of English composition rightfully takes in all our work, elementary or advanced. In the sense which I take it, the study of English composition is, and must be, rather a serving-maid than a mistress, rather a means than an end. Rhetoric is nothing more nor less than the art or power of expressing plainly, clearly, and adequately, the thing which you most want to express. It is an art, then, which any boy of good sense can almost learn for himself — in fact, does usually learn for himself, if he learns it at all. The function of the instructor in English composition in a preparatory school, as well as in the college, is merely that of helping a boy, systematically and with a proper modicum of theory, to a boy's natural birthright — that of being able to say what he wants to say, or to write what he wants to write.

There are several obvious faults which a diligent reading of examination books shows in the early training in English of candidates for admission to colleges and technical schools. The first fault is that the boy has been taught to write as well as he could only when he wrote for his English master. To the ordinary schoolboy English composition is too much a matter of a fixed hour and a fixed place, and not a matter of everywhere and always. He hands in his composition on Friday, gets it back again on Wednesday, and neither in the interim thinks anything further about trying to write well, nor afterwards applies on other occasions what help he might have received from that particular exercise. But English composition — the art or power of making another man understand what you mean through written words — is not an isolated act, like attendance at a yearly convention, but a habit, a constant series of acts. If we teach a boy to write well — that is, to express what he has to say briefly, clearly, and accurately — we must teach him to

write well not only on the rarer occasions of fortnightly themes, but on all occasions — in his examination papers, in the note-books of the physical or chemical laboratory, in translating into English from other languages, in mathematical demonstrations, in absolutely all the work which he does. Good writing is good thinking, good thinking out into language, and applies as well to work done under the teacher of physics as to that done under the teacher of English.

A second fault which I notice in the English preparation of sub-freshmen, as I believe they are technically called, is that they have usually been trained to write on too large subjects. A boy writes best about things which he knows about. But "Hope," "Fortune," "Youth's Best Lesson — Obedience," "The Proverbs of the World" — these, I submit, are usually subjects out of the reach of boys fitting for college. The following titles, on the contrary, taken absolutely at random from a bundle of freshman themes, will indicate that in the majority of cases the writer had chosen a subject in which he was really interested, about which he really knew something, and on which he was likely to write with success. These are the titles: "Boston Winds in December," "The Blue Jay," "The Advantages of a College Education," "Sunday Morning," "The Alternating Current Dynamo," "A Moral Meeting," "The Mechanics' Fair," "Some Facts about the Telephone," "A Ride on a Raft," "A Blockade," "Safety Bicycles," "A Fishing Trip."

A third fault, somewhat less important than those I have mentioned, but perhaps nearly akin to them, is that a boy is often set to writing about books before he is ready to be of his own accord interested in them. Such a practice only confirms the pupil in what is too often his instinctive belief, that composition is merely a scholastic exercise, only applicable to books, and with no relation to the half-dozen kinds of writing which every boy practices in his daily life. To be sure, the subjects given the candidate at the time of the examination are taken from certain well-known books in English literature, the list of which is published in the catalogue of the institution. We choose such subjects because it is the uniform practice of New England colleges to require a knowledge of these books for admission, and because a teacher preparing boys for both colleges and the Institute of Technology should not be forced to divide his class into two squads, as if there were an appreciable difference between the amount or kind of drill in English needed

for the Institute, and that needed for Harvard or any other reputable American college. We choose them, too, because it is the easiest way of providing subjects with which a candidate can make himself familiar, and because the reading of good books brings obvious advantages. But, after all, the examination is one not in literature, but in expression, and in preparing for it care should be taken not to make the pupil write in the conventional school composition style about books, and, if possible, not to write about them at all until they begin really to interest him. He should be taught to have his own ideas (not necessarily those of his teacher) about what he reads, and to be used to writing them down. The more simply he writes, the less artificiality of thought he acquires, the greater the advantage in power of expression is likely to be.

The only other prominent fault which I have noticed is a strong tendency in candidates, sometimes in the best candidates, to write not as boys should, but in a fashion more appropriate to Johnson, Macaulay, or the *Edinburgh Review*. This bookish trait, which it is one of the first duties of a college instructor to eradicate, is illustrated by the following exact transcript of an essay written at an admission examination:

"My course in English for the last four years has been carried on in the X—— school building at Y——. This course was *admirably arranged* by Madame Z——, *a woman of great literary ability and a keen perception of what is lacking and of what is necessary for the superficial scholar of this day.* In this course the writing of composition is the most prominent feature. At the end of each term during the year a 'long' composition is given (subject stated) to the student to write, while every second week throughout the year an abstract is written in the class from memory. The subject of this abstract is taken from *some great literary production, the substance of which is committed to memory, and rewritten in an abridged form after the style of the author.* The character of this drill is very simple at first becoming harder each succeeding year; when at the end of the course, one finds out that he is not only well informed on a good many subjects, but through the constant practice of abstract writing he expresses himself with much readiness and facility and *through the medium of the long composition he has become capable of deep thought and analysis of large subjects.*

"Important as composition writing is, time is found for the pursuit of other subjects. I am compelled to read and be questioned on the standard English poets, essayists, criticts and novelists, with particular attention to their style. Accompanying this work we must commit to memory fragments of the productions so we take them up in turn, and have some practice in the analysis of these subjects without expanding them.

"Accompanying but not directly connected, with the study of Literature is a very elementary course in philology. On one day out of the week I am compelled to look up the history of ten words in Skeats dictionary, with a few Aryan and Teutonic roots to memorize.

"Should this course seem a little desultory, or too comprehensive for an ordinary High school, it must be understood that five hours a week are given to it, and that by far the greater part of the work is done outside of school at the students will.

"*With respect to my direct preparation for this examination I have resorted to home studying and reading.* I have kept up a constant course of reading for the last year; principally with the standard novelists. But in many cases I conflict with my school course where I am compelled to sacrafice my private reading. *It is unnecessary to say that I have resorted to a book on Literature for a more complete preparation of the standard books and authors* which we would not be likely to obtain in a small *home* library."

The sentences which I have italicized will perhaps indicate most plainly the real difficulty. Almost every line shows the traces of the method in which the writer has been drilled. The style which he had been forced to cultivate was not one which he could slowly work out for himself, and which should always be natural. "The subject of his [this] abstract is taken from some great literary production, the substance of which is committed to memory, and rewritten in an abridged form after the *style* of the author."

2. The principle which underlies the other part of our examination — that of correcting specimens of bad English — is a simple one. Here we test not expression, but knowledge. Rhetoric really consists of questions of taste and questions of absolute right and wrong. Drill in matters of taste we can afford to leave, for the most part, until the student is a freshman in college. There he can best attain skill in the exact choice of words with all their fine shades of meaning, in deftly framing them into well-balanced sentences, in coherent logical structure both of paragraphs and of the larger units of composition, in all that sound and nice thinking out into language which makes a man write clearly, forcibly, and pleasantly. But there is much in writing which is not a matter of taste, but of knowledge. And that knowledge a candidate should have when he presents himself for the admission examination. He should spell correctly, he should be familiar with the main principles of punctuation, he should know the difference between *shall* and *will* and *lie* and *lay*, and, in general, he should be exactly informed about plain questions of syntax and idiom about which there can be no doubt. That an average candidate does not have this exact knowledge the reader will find by examining the following random extracts from the essays written at an admission examination:

"On the shoar of the Arcadian land, the small village of Grand-Pré was siduated: . . . here, too, her farther lived and worked on his fertil farm; at peace with his neighbors, with all the world."

"In preparing for the Institute this fall, I was only studying my 'conditions,' of two years ago, when I was told I would have to take all of the examinations over, and the result will be, that I will be 'conditioned' in six, and perhaps seven studies."

"In the grave yard in Philadelphia, there lies to nameless graves side by side."

If boys will write in this fashion, the examiner must find some means of testing their knowledge point-blank. This he does by printing a dozen or so sentences, in each of which there are palpable errors, and asking the candidate to correct them. If he cannot detect common blunders when they are so isolated as to provoke discovery, it is not likely that he will be successful in expelling the same errors when they occur — *currente calamo* — in his own composition.

Such are the principles, as I understand them, which underlie the requirements in English for admission to the New England colleges and to the Massachusetts Institute of Technology. For the advantage both of the college and the preparatory school these requirements should be as definite as possible. In almost all other subjects it is well understood where a student's preparation should begin and where it should end. It is only when the requirements in English shall likewise be definitely understood that the teaching of English in colleges or in schools can have the best, or even good, results.

THE SPECIFIC ORGANISM OF TYPHOID FEVER

(*BACILLUS TYPHOSUS—EBERTH*).

By GEORGE W. FULLER, S.B.

Since the recognition of the germ theory of disease, typhoid fever is generally attributed to the presence of a definite organism, namely, the typhoid fever bacillus. This organism exists sometimes in air, milk, soil, and water, and consequently is highly interesting to the sanitarian. With the occurrence, from time to time, of typhoid fever epidemics, the need and importance of its study is felt, particularly by those in charge of water supplies. The different phases presented in these epidemics render this study of interest not only to the bacteriologist but to the sanitary engineer and the sanitary chemist.

At the suggestion of Professor Sedgwick, I propose to record the principal literature upon the typhoid fever bacillus, citing its history and specific characteristics.

History.

The infectious nature of typhoid fever was recognized long before the dawn of bacterial science and the discovery by Eberth of the specific germ. The studies of the progress of epidemics, by Galen, Huxam, Pringle, and Frank, showed danger in putrefying animal matters, and gave rise to the expression, *the source of contagion*. Budd[1] and Murchison[2] proved that the cause of typhoid fever sometimes existed in air, and particularly in potable waters contaminated by fæcal matter or the products of putrefaction. To Murchison, with his pythogenic theory already in existence, the presence of putrefying matter seemed sufficient; but not to Budd, who concluded "that typhoid fever is in its essence a contagious or self-propagating fever; and that the living body of the infected

[1] Typhoid Fever; its Nature, Mode of Spreading, and Prevention, London, 1873.
[2] Treatise on the Continued Fevers of Great Britain, London, 1862.

man is the soil in which the specific poison, which causes the fever, breeds and multiplies. Reproduction of this poison in the infected body, and the disturbance attaching to it, constitute the fever." This great observer was, accordingly, an early exponent of the germ theory of disease, and rigid bacteriological researches in subsequent years have confirmed his theory.

In 1864 Tigri announced to the Academy of Sciences that he had found numerous bacteria in the pulmonary veins, and particularly in the left heart of a man dead of typhoid fever. His observations were confirmed by Mégnin[1] two years later.

Coze and Feltz,[2] in 1866, in their work on infectious diseases, described bacilli which they found in blood. They were $2-5\,\mu$ (one thousandth of a millimeter) long, segmented, slightly motile, and morphologically resembled the *bacterium catenule* of Dujardin. Hallier,[3] in 1868, described as typhoid germs two micrococci of different size.

In studying a complicated case of typhoid fever and of miliary abscess of the kidney, Recklinghausen[4] found some microörganisms to which he laid no claim as to pathogenic value, but of which he remarked that, if there was an active principle of typhoid fever in existence, it could be found in other parts than the blood. Modern science has shown that he simply found some microörganisms of suppuration, introduced by secondary infection.

Eberth,[5] in 1873, in his first work on typhoid fever, endeavored to reconcile these varying opinions. He found microörganisms only in internal ulcerations on the point of healing or of mortification. The organisms described in the blood or internal organs of typhoid fever victims he considered to be introduced secondarily into the system.

In 1875 an English writer, Klein,[6] described some organisms found in the intestines, mesenteric glands, and fæces of typhoid fever victims, as micrococci, varying in size from that of a fine granule to double that of a blood globule. Isolated, or in pairs, says the author, they have in general the configuration of a sphere, of a sand grain,

[1] Comptes rendus, Académie des Sciences, Paris, 1866.
[2] Gazette Médicale de Strassbourg, 1866.
[3] Virchow's Archiv, 1868.
[4] Verhandlungen der medicinischen Gesellschaft, Wurtsburg, 1871.
[5] Zur Kenntniss der bacterischen Mykosen, Leipzig, 1873.
[6] Reports of Medical Officers, London, 1875.

or of a bean. This article brought the writer great renown in England. To-day the micrococci of Klein are classified among the ordinary and numerous species of micrococci which inhabit the intestines.

Browicz,[1] Sokoloff,[2] and Fischl[3] described as typhoid bacteria some oval micrococci found in the kidneys, spleen, and intestines of typhoid fever victims. They were only organisms of putrefaction, however. These works were all futile, abounding in contradictory definitions; the typhoid organism was yet to be found.

Eberth, in a series of articles published in 1880 and 1881, in Virchow's Archiv, first described the organism which now bears his name. Whether it was identical with the one previously described by him in 1872, it is impossible to say, as in former work his description was insufficient to characterize it. But with the introduction of aniline colors into bacteriological technique, he was able to note some of its morphological characteristics. He found the organism, which is now generally considered to be the pathogenic agent of typhoid fever, in the mesenteric glands and spleen of typhoid fever victims, but never encountered it in the blood or organs of persons affected with other diseases. It is a bacillus, says Eberth, rounded at the ends, elongated, sometimes oval, containing sometimes in its interior small dull bodies resembling spores, one to three in each bacillus; takes the aniline colors very badly, and occurs in sections in masses of varying number. This now appears to have been a great discovery. Although he merely described some of the morphological characteristics, yet the bacillus could be differentiated, in a certain measure, from other organisms.

In the same year, 1881, Klebs[4] confirmed the discovery of Eberth, but found, moreover, some elongated filaments in nearly all the organs of typhoid fever victims. These he considered to be bacilli of Eberth in a more advanced stage. Eberth's investigations were again confirmed by Koch,[5] who condemned the work of Klebs, considering the filaments foreign to the disease. Confirmation was

[1] Comptes rendus, Académie de Cracovie, 1875.
[2] Virchow's Archiv, 1876.
[3] Prager medicinische Wochenschrift, 1878.
[4] Archiv für experimentelle Pathologie, 1881.
[5] Mittheilungen aus dem Kaiserlichen Gesundheitsamtes, Bd. I.

also given by Mayer and Friedlander[1] in Germany, and by Coates[2] and Crookes[3] in England.

Morphological characteristics alone, however, are not sufficient for classification. The bacteriological methods at this time were crude, and known to very few investigators. But a great step was soon taken in bacteriological progress, namely, the invention by Koch of the solid gelatine peptone culture medium now in general use. By its means Gaffky[4] was able to obtain a pure culture of typhoid bacilli, to study their growth, spore formation, and other morphological characteristics leading to classification. The work of Gaffky was confirmed in France by Cornil.[5]

The methods for the differentiation of the typhoid bacillus were now fairly well established. In 1886 the infection of the lower animals by this organism was carefully studied, as will be shown later in detail.

Specific Characteristics.

Morphology.— The typhoid organisms, when examined in a hanging drop under the microscope, are seen to be bacilli, two to three times as long as broad, possessing an energetic movement. For detailed study, staining is necessary. The typhoid bacillus, unfortunately, is stained with some difficulty. This is probably due to the ease with which it is decolorized in the necessary processes of preparation. It is best stained by Ziehl's[6] and Loeffler's[7] solutions. A thin film of bacilli is placed on a cover slip by the aid of a sterilized platinum needle, dried in the air and passed three times through the flame of a Bunsen lamp. The coloring agent is now added for a few minutes. Great care is necessary in removing the excess of coloring agent; otherwise the bacilli will be faintly or not at all stained. The excess is best removed by filter paper or by a fine stream of water directed from a wash bottle upon the edge

[1] Untersuchung über den Bacillus des Typhus abdominalis, Berlin, 1881.
[2] British Medical Journal, March 25, 1882.
[3] *Ibid.*, July 1, 1882.
[4] Mittheilungen aus dem Kaiserlichen Gesundheitsamtes, Bd. II, pp. 372–403.
[5] Journal des Connaissances Médicales, 1884.
[6] 100 grms. distilled water,
5 " carbolic acid (crystals),
10 " alcohol,
1 grm. fuchsine.
[7] 30 cc. conc. alcoholic solution of methylene blue,
100 cc. of 0.01% KOH solution.

of the cover slip. The preparation is then dried and mounted in balsam.

Upon examination the bacillus generally appears $1-3\ \mu$ long, $0.4-0.7\ \mu$ in diameter, and rounded at the ends. Its size is not constant. No organism, says Gasser,[1] is so polymorphous as the typhoid bacillus, even when it is grown in the same culture medium. It not only appears to be a bacillus of very varying dimensions, but has been observed by Litzerich[2] to resemble a micrococcus. From old cultures grown in bouillon or sterilized milk it measures $5-8\ \mu$ in length and $2-3\ \mu$ in diameter. Vaillard and Vincent[3] grew the typhoid bacillus on human spleen bouillon, and found it to measure $120\ \mu$ in length. The possible objection, that it was not a pure culture of the typhoid bacillus, was removed by the fact that the bacillus easily resumed its normal size and characteristics when transferred to agar.

Examination frequently shows also a round or oval space in the center of the bacillus which is not stained. This was noted by Eberth,[4] Mayer and Friedlander,[4] Artaud, and was first carefully studied by Gaffky.[4] Artaud[5] found this to be a constant characteristic in preparations taken from organs of typhoid fever victims; and without studying it when grown on the different culture media, with which he was but little familiar, hastily concluded that this was a means of differential diagnosis from other bacilli. Gaffky's researches on its growth on gelatine showed it to almost constantly possess the cylindrical form. On potato, however, it gave the "shuttle" appearance.

Several reasons have been advanced in explanation of this non-stained space in the center. Artaud considered it due to the presence of a central spore, and claimed to have seen one or two small spores within the space. Other writers have suggested that the stained ends were spores. Chantemesse and Widal[6] attributed it to the partial degeneration of the center of the bacillus. In multiplication the bacillus divides at this point. In support of their theory, they have been able to produce this result artificially, by the growth

[1] Archives de Médecine Expérimentale, January, 1891.
[2] Archiv für experimentelle Pathologie, Bd. IX, p. 312.
[3] Referred to by Gasser, *loc cit.*
[4] *Loc. cit.*
[5] Étude sur l'étiologie de la fièvre typhoïde, Paris, 1885.
[6] Bulletin Société d'Anatomie, December, 1886.

of the bacillus on medium of diminished nutritive power. The typhoid bacillus grown on gelatine acidified with a few drops of a 5 per cent. solution of carbolic acid, which retards its development, showed a non-stained space in the center; while bacilli taken from the same original culture, when grown on ordinary gelatine in the same length of time, gave the cylindrical form.

Moreover, Chantemesse and Widal[1] have found several water species, and Blocq[2] has discovered an air species, which present such a non-stained center. Although this characteristic does not possess the diagnostic value attributed to it by Artaud, it is not to be neglected. On account of its appearance when the bacillus is grown on potato and acidified gelatine, it is entitled to a place in the series of characteristics which enable the bacteriologist to identify the bacillus of typhoid fever. This point will be again alluded to under spore formation.

Motility.— The energetic motion, when examined in a hanging drop, is one of the most important characteristics of this bacillus. It not only moves about in the field of the microscope, but has a distinct vibratory motion around its axis. This was thought to be due to cilia, as was shown to be the case with large motile bacteria by Koch and Neuhauss. By means of a special staining method, Loeffler[3] was able to show the presence of cilia on the typhoid bacillus. He employed a mordant made by adding, drop by drop, to 10 cc. of a 20 per cent. aqueous solution of tannin, an aqueous solution of ferrous sulphate, till the liquid is violet black; three or four cc. of logwood (one part logwood to eight parts water) are added, giving the liquid a marked violet tint; 5 cc. of a 5 per cent. solution of carbolic acid serves to preserve the solution without affecting its power as a mordant. His stain was made by adding to a saturated aqueous solution of aniline, 1 cc. of a 1 per cent. solution of NaOH and 4 to 5 grams of methylene blue or fuchsine, as desired. The coloring material goes into solution on shaking, and the solution is then sterilized. Cover slips are prepared in the usual manner, the mordant added and heated gently till steam is evolved. The mordant is then washed off, two or three drops of the coloring agent added and heated gently until

[1] Archives de Physiologie, March, 1887.
[2] Referred to by Chantemesse and Widal.
[3] Centralblatt für Bakteriologie, 1888, p. 209. Referred to by Gasser, *loc. cit.*

sufficient coloration is obtained. The preparation is then washed, dried, and mounted.

More recently Loeffler[1] has confirmed his work, finding the typhoid bacillus to possess several cilia, very fragile, easily detached, and appearing particularly in cultures five to eight hours old, on freshly coagulated blood serum. Babes[2] has confirmed Loeffler's investigations, finding the bacillus to possess five to twenty cilia, sometimes found in bunches at one end or side, and sometimes distributed unevenly along the sides or ends.

SPORE FORMATION.— The formation of spores by the typhoid bacillus has been much discussed. Gaffky[3] found at the end of the bacillus, small, clear, refracting spheres, which were not stained by aniline colors. These forms occurred regularly when the bacillus was grown at 37° C. on potato or agar. After drying many months it showed a luxuriant growth when transferred to culture medium. This admitted, at least, the possibility of true spore formation; and, after comparison with the spore formation in other species, led Gaffky to conclude that they were true spores. However, the shining bodies of uniform size within the bacillus, which one is accustomed to see, were wanting.

Seitz,[4] after growing the bacillus on potato, bouillon, and agar, two to five days, at the body temperature, was unable to confirm Gaffky's observations. Chantemesse and Widal[5] grew the bacillus on potato at 38° C., and after four or five days observed, at the ends, oval-shaped bodies of a diameter greater than that of the bacillus itself. They were not affected by the aniline colors.

Buchner[6] showed that the typhoid bacillus, grown at 37° C. on potato, presented at its extremities clear, refracting spheres, apparently spores. He also proved that these spores were more readily formed when the bacillus was cultivated on acid potato, agar deprived of oxygen, and other media not favorable for growth. This led him to think that a form of involution existed. The so-called terminal spores he found to stain more intensely and more rapidly than the bacillus itself. He concluded from his experiments that there was

[1] Centralblatt für Bakteriologie, 1890, p. 625.
[2] Zeitschrift für Hygiene, Bd. IX, p. 323.
[3] *Loc. cit.*
[4] Centralblatt für klinische Medicin, 1887, p. 267.
[5] Archives de Physiologie, March, 1887.
[6] Centralblatt für Bakteriologie, 1888, pp. 353 and 385.

no spore, but a contraction of protoplasm; and that in consequence, either at the ends or at the center of bacillus, *after staining*, non-stained vacuoles were formed.

This explains the non-stained, central vacuoles mentioned above, which caused much controversy among bacteriologists during the preceding years.

Further biological argument was shown by the fact that these bacilli offer less resistance to drying and the action of heat than those not possessing this non-stained vacuole formation. This work was verified by Pfuhl,[1] who arrived at the same conclusions.

Vaillard and Vincent[2] cultivated the typhoid bacillus, obtained from the spleen of a typhoid fever victim, on human spleen bouillon. They found at the end of sixty days a large number of bacilli of varying length, the greater number exceeding 120 μ, possessing a linear series of bright refracting bodies not stained by the aniline colors. The bacillus grown in the same bouillon did not show these bodies after sixteen days. They found that these vacuolated filaments were killed by heating for ten minutes to 54° C., while the ordinary typhoid bacillus was killed by heating for ten minutes to 55° C.

Birch-Hirschfeld[3] found that 1 cc. of 1 per cent. solution of rosaniline, added to 10 cc. of nutrient gelatine, colored rounded masses within the bacillus. This he thought favored the idea of true spore formation.

The indications at the present time, however, resulting from morphological and physiological experiments, are that the bacillus does not form true spores.

Biology.— Janowski[4] has contributed some interesting experiments on the influence of light and temperature upon the typhoid bacillus. After inoculating a number of gelatine tubes, he placed them in a cool room, near a window through which the sun did not shine, and wrapped a portion of them in a dark, thick paper. These latter he found showed a growth in three days, while those exposed to the rays of light required five days for the appearance of growth. After six hours' exposure to the direct rays of the sun, freshly inoculated bouillon proved sterile. To show that no chemical change

[1] Centralblatt für Bakteriologie, 1888, p. 769.
[2] *Loc. cit.*
[3] Archiv für Hygiene, 1888, p. 341.
[4] Centralblatt für Bakteriologie, 1890, Bd. VIII, Nos. 6–9, 14 and 15.

had taken place in the medium, he again inoculated the same bouillon under the usual conditions, and obtained the normal growth. That this was not due to heat was indicated by the fact that the maximum temperature never exceeded 40.5° C. — but little above its optimum temperature.

The author concluded that this effect was produced by the chemical action of direct sunlight. In support of this, he placed tube cultures in flasks containing a solution of bismarck brown or of potassium bichromate, through which the blue, violet and ultra violet rays do not pass, and in all cases obtained the normal growth; while tube cultures placed in flasks containing solutions of fuchsine or methyl violet, through which the blue, violet and ultra violet rays pass, proved sterile.

For studying the action of high temperatures on the typhoid bacillus, he used a double-walled, felt-covered water bath, by means of which he could maintain a constant temperature for fifteen minutes. The apparatus was so arranged that the tube of gelatine could be inoculated while remaining in the water bath. After removal from the hot water, the tubes were rolled after Esmarch's method. In this manner Janowski found that the bacilli were killed usually in ten minutes at 56° C., and in all cases in ten minutes at 57° C. These results agree with those of Sternberg.[1] Pfuhl, Buchner, and others had obtained a somewhat lower temperature, but their method — placing the bacilli on a sterilized needle in hot air — was less accurate.

He confirmed Prudden's[2] statement that the bacillus is killed by alternate freezing and thawing of the culture in a mixture of calcium chloride and ice.

The actual effect of winter weather was studied by placing a bouillon culture, January 17, in snow, which then covered the ground, and daily removing a small portion of the frozen bouillon for examination. With the changing temperature of the air, the bouillon was frozen and thawed many times, but showed a growth until February 5. The night of February 4 was extremely cold, the temperature of the air being lower than 0° C., while that of the snow was 14° C. The culture showed no further growth. Prudden, however, found the bacillus capable of growth after having been frozen in ice for 103 days.

[1] Medical News, April 30, 1887.
[2] Medical Record, 1887.

The life and growth of the typhoid bacillus in water, sewage, and soil, has not as yet been thoroughly studied. A clue to the present state of our knowledge upon the subject can be obtained from the following brief outline:

The typhoid bacillus apparently does not multiply in water, sewage, or soil. In sewage the duration of life varies from four days[1] to three months.[2] In soil it has been found by Grancher and Deschamps[3] after 5½ months, and in the region of a suspicious well it was found by Macé[4] at a depth of 2 – 3 meters. The varying length of life is not fully understood. Karlinski[5] concludes that temperature does not affect continuation of life. Straus and Dubany[6] infer from their experiments that the chemical composition of a water possesses no appreciable influence upon the duration of the life of the typhoid bacillus in the given water. The majority of investigators in this subject, particularly in its relation to drinking water, however, have failed to face the problem as it exists in nature; that is to say, having worked with sterilized water and with sterilized sewage, they have wholly overlooked the complications which arise with the presence of the ordinary species of water and sewage microorganisms.

THE CULTIVATION OF THE TYPHOID BACILLUS.

The typhoid bacillus can be grown in culture media at temperatures varying from 10° C. to 46° C.[7] Its optimum temperature, however, lies between 37° C. and 39° C.

It is a facultative anaërobe; that is to say, it grows most readily and most characteristically in the presence of oxygen, although the absence of oxygen, while retarding its development, does not wholly prevent its growth.

The typhoid bacillus shows a growth on the gelatine plate in the usual time, 48 – 72 hours, at the temperature of the room. The colonies lying deep in the gelatine are small, white, circular or spindle-shaped, and sharply outlined. The surface colonies are larger,

[1] Schiller, Arbeiten aus dem Kaiserlichen Gesundheitsamtes, Bd. VI, p. 197.
[2] Uffelmann, Centralblatt für Bakteriologie, 1889, Bd. V, p. 497.
[3] Archives de Médecine Expérimentale, T. I, p. 33.
[4] Comptes rendus, Académie des Sciences, T. CVI, p. 1546.
[5] Centralblatt für Bakteriologie, Bd. VI, No. 3, p. 65.
[6] Archives de Médecine Expérimentale, T. I, p. 5.
[7] Gasser, *loc. cit.*

spreading out into a thin, bluish, transparent skin, with irregular outline, often appearing yellowish in the center; and when seen under the microscope usually exhibit markings similar to the veining of a leaf. When viewed by transmitted light, the colonies give a yellowish brown color. They are very similar to the colonies of coli commune — a species common in fæces. The similarity is even more marked when examined under the microscope. Coli commune, however, usually grows more quickly and forms larger colonies.

When grown in a gelatine tube, small round colonies, very closely joined together, appear along the inoculation line. At the surface appears a thin, grayish, shining skin, which spreads to the wall of the tube. When a tube of melted gelatine is solidified so as to give an inclined surface, the surface growth following inoculation can be well observed. On both sides of the inoculation line it gives a thin, nearly transparent, shining, bluish-white coating of irregular outline. The intensity of growth is somewhat variable, and is attributed by Seitz to the greater or less alkalinity of the culture medium. Garré[1] observed a curious feature of its growth on gelatine, namely: if all growth is removed by a sterile needle from a tube, and the tube again inoculated, it remains sterile — the gelatine being apparently "vaccinated." This has been confirmed by several French bacteriologists.

On agar and blood serum it gives a moist, white growth, showing no special characteristics.

In bouillon it forms a slight uniform turbidity and considerable yellowish-white dusty sediment.

It is on sterilized potato that it shows its most marked characteristic. In two days at the body temperature, or three to four days at room temperature, the inoculated potato gives a luxuriant but to the unaided eye completely invisible growth. The entire surface of the potato assumes a moist, shining appearance, but is not otherwise changed. If a small portion is removed with a sterilized platinum needle, and examined in a drop of water under the microscope, a large number of bacilli, moving about very energetically, are to be seen. It was thought at one time that this was a method of differentiation from all other species. Several investigators, however, have found that this characteristic is obtained only when the bacillus is grown on slightly acid potato; slightly alkaline potato gives a

[1] Correspondenzblatt für Schweizerische Aerzte, 1887.

gray or yellow growth along the inoculation line. This point has been carefully studied by Schiller,[1] who confirmed the variable growth on potato. Moreover, a few species, particularly water species, have been found which give a similarly invisible growth on potato, showing that this is not an absolute method of differentiation.

SPECIAL METHODS AND MEDIA FOR CULTIVATION.

Many efforts have been made to devise some new method of procedure which shall furnish unfailing means for identification. Chantemesse and Widal[2] used nutrient gelatine, containing 0.2 per cent carbolic acid, which they claim prevents the growth of nearly all other species, but does not retard the growth of the typhoid bacillus. Thinot[3] added 0.25 grms. of carbolic acid to 100 cc. of the water which he wished to examine. Petruschky[4] considered the formation of acid to be of diagnostic value. Holz[5] added 10 per cent. of gelatine to the juice of raw potatoes, obtaining a solid, transparent, slightly acid medium, on which the typhoid bacillus grows quite characteristically, while nearly all other species refuse to grow, particularly if 0.05 per cent. carbolic acid is added. Kitisato[6] found that typhoid and some other species (he gives a list) give a negative indol reaction, while many water and sewage species produce the characteristic red coloration, when to a bouillon culture grown for 24 hours at 37° C. are added 1 cc. of a solution containing 0.02 grm. $NaNO_2$ in 100 cc. and a few drops of concentrated H_2SO_4. This fact he considers of much diagnostic value in some cases.

These methods have all been carefully studied, and none have proved to be a trustworthy means for the identification of the typhoid bacillus.

Gasser[7] has recently made some experiments with colored gelatine. He found only two species — coli commune and typhoid — which decolorized gelatine colored by a few drops of fuchsine. These can be readily differentiated, he says, by the fact that coli commune

[1] Arbeiten aus dem Kaiserlichen Gesundheitsamtes, Bd V, p. 312.
[2] Archives de Physiologie, March, 1887, p. 217.
[3] La Semaine Médicale, 1887, No. 14, p. 135.
[4] Centralblatt für Bakteriologie, Bd. VI, 1889, p. 660.
[5] Zeitschrift für Hygiene, Bd. VIII, p. 148.
[6] Zeitschrift für Hygiene, Bd. VII, p. 515.
[7] Archives de Médecine Expérimentale, November, 1890.

decolorizes the gelatine in two to three days, while typhoid requires six to eight days. Moreover, on colored agar the coli commune growth is confined to the inoculation line, while the typhoid growth is diffused through the neighboring medium. I have tried this method, but was unable to find decolorization by the typhoid bacillus, even after allowing the inoculated plates to remain sixteen days.

Noeggerath[1] has proposed the use of gelatine colored by the following mixture of aniline colors:

Saturated aqueous solution of methylene blue	2 cc.
" " " gentian violet	4 "
" " " methyl green	1 "
" " " chrysoidine	4 "
" " " fuchsine	3 "
Distilled water	200 cc.

The typhoid bacillus gives complete decolorization in fifteen days. This method has been tried by different investigators with varying results, and definite conclusions as to its real value have not yet been reached.

Rodet[2] has suggested cultivation at 44.5° C. This temperature enables the typhoid bacillus to grow, but destroys nearly all other organisms.

Vincent[3] gives a modification of this method, which is especially adapted to the examination of a suspicious drinking water. He uses bouillon containing one drop of carbolic acid to every 2 cc. After mixing with the water he cultivates at 42° C. If, after twelve hours, the solution remains clear, he claims that the typhoid bacillus is not present. But if a turbidity appears, he transplants a small portion to a fresh tube, and so on, until he obtains a pure culture, from which he learns the characteristics of the suspected organism after the methods outlined above.

Péré[4] has very recently published a similar method, which he used with success in the examination of service water at Alger. Into a liter flask he puts 100 cc. of neutral, sterile bouillon, containing equal parts meat juice and water; 50 cc. of a neutral, sterile, 10 per cent. peptone solution; 600 to 700 cc. of water to be examined; 20 cc. of a 5 per cent. carbolic solution, and then fills the flask with

[1] Fortschritte der Medicin, 1888, Bd. VI.

[2] Comptes rendus, Société Biologique, T. II, No. 8. Blyth's Manual of Public Health, p. 496.

[3] *Ibid.*, T. II, No. 5. *Ibid.*, p. 497.

[4] Annales de l'Institut Pasteur, 1891, p. 79.

the suspected water. He then puts this liquid into ten sterile flasks, and keeps them at approximately 34° C.— within the limits of 32° C. and 36° C. From this point his method is identical with that of Vincent. He claims that coli commune and the typhoid bacillus are the only organisms which develop by this procedure.

It is very important to note that the growth of the typhoid bacillus on each of the different culture media is more or less variable; and a definite opinion concerning its identity can be obtained only from a careful study of its growth on all the culture media. Moreover, Babes,[1] in his careful study on the "Variability and Varieties of the Typhoid Bacillus," states that the bacillus becomes larger when grown on gelatine and potato than when grown on agar. Many times he found the gelatine and potato cultures contained bacilli 0.7 μ in diameter.

INOCULATION OF ANIMALS.

The first experiments on the inoculation of the lower animals with pure cultures of the typhoid bacillus were made by Gaffky.[2] He infected more than sixty animals, including guinea pigs, monkeys, rabbits, white rats, gray mice, field mice, pigeons, chickens, and calves. His inoculations were made in the veins or in the peritoneal cavity. After a long time death followed, in some cases from a general breaking-down of the system; but in no case did the autopsy show any connection with typhoid fever.

On the other hand, E. Fraenkel[3] and Simmonds inoculated in the auricular veins a large number of rabbits; one half of them died in twenty-four to forty-eight hours, showing a swelling in the spleen, mesenteric glands, and intestinal follicles. Examination showed the bacillus in the spleen, but they did not usually pass into the intestines.

Iwan Michael,[4] Fodor,[5] and Seitz,[6] confirmed these experiments. Seitz, moreover, prepared some guinea pigs after the method used by Koch to facilitate cholera infection; that is to say, he rendered the contents of the stomach alkaline with sodium carbonate, and

[1] *Loc. cit.*
[2] *Loc. cit.*
[3] Die Aetiologische Bedeutung des Typhus-Bacillus, Hamburg, 1886.
[4] Fortschritte der Medicin, 1886, p. 353.
[5] Deutsche medicinische Wochenschrift, 1886, No. 36.
[6] Bakteriologisches Studium zur Typhus Aetiologie, München, 1886.

retarded the movement of the intestines by opium. Into the stomach of part of them he injected the fæces of a typhoid patient. The majority died and gave symptoms identical with those of the animals which died from an injection of a pure culture of typhoid bacilli. The spleen and mesenteric glands were swollen, and the intestines much changed. The intestines and some of the other organs contained typhoid bacilli; the blood was free. Into the remainder of the animals, however, he injected the fæces of a person in good health, and no effect was observed.

A. Fraenkel[1] also confirmed these results, inoculating fourteen guinea pigs in the duodenum, after the method of Nicate and Rietsch. He obtained seven positive results — the animals dying in three to seven days. The autopsies showed the condition of intestines and other organs to be similar to those mentioned above. He also noted in one case a round ulceration a half centimeter in diameter.

Contradictions were not slow to appear. Sirotinin[2] and Beumer and Peiper[3] simultaneously published results directly opposed to the above. Sirotinin did not deny that animals inoculated with typhoid bacilli died after a few hours; but he claimed that death was not caused by typhoid fever, but by a poisoning by the ptomaïne. He found that the ptomaïne extracted from sterilized cultures produced the same effect as the living bacilli. The success of the experiment depends upon the quantity of material injected, the chemical composition of the culture medium, and the age of the culture. The increase in the number of bacilli, noted by former investigators, he was unable to confirm. He claimed that the injected bacilli pass rapidly away, and an increase in numbers is to be found only when the autopsy is delayed, thereby enabling the few remaining bacilli, in such organs as the spleen, to multiply rapidly.

Beumer and Peiper confirmed these conclusions and found, moreover, that death could be produced in mice, rabbits, and guinea pigs, by the inoculation of ordinary water species such as bacillus subtilis, micrococcus prodigiosus, white bacillus,[4] and the liquefying fluorescent bacillus.[5] They conclude, without hesitancy, that it is not possible to produce typhoid fever in the lower animals.

[1] Centralblatt für klinische Medicin, 1886, No. 10.
[2] Zeitschrift für Hygiene, Bd. I, p. 465.
[3] *Ibid.* Bd. I, p. 489.
[4] No. 19, Eisenberg's Tabellen (Erste Auflage).
[5] No. 6, Eisenberg's Tabellen (Erste Auflage).

PTOMAÏNE.

The discovery of ptomaïnes (alkaloids formed by bacteria during the putrefaction of organic matter) opened a new and important field in experimental medicine. The ptomaïne of the typhoid fever bacillus was discovered by Brieger,[1] and named typhotoxine. He obtained it by cultivating the bacillus on beef broth for eight to fourteen days at the temperature 37.5° C. – 38° C. The following outline of his method of procedure is taken from Vaughan and Novey.[2]

The beef broth culture is first treated for a few minutes with water slightly acidified with hydrochloric acid, care being taken that the feebly acid reaction is maintained. The liquid is then filtered and concentrated, at first on a plate and then on a water bath, to a syrup; an extraction made with 96 per cent. alcohol, and the filtered extract treated with a warm alcholic solution of lead acetate. The lead precipitate is removed by filtration; the filtrate evaporated to a syrup and again extracted with 96 per cent. alcohol. The alcohol is driven off and the residue taken up with water. After removal of traces of lead by hydrogen sulphide, the filtrate is acidified with hydrochloric acid and evaporated to a syrup. This syrup is extracted with alcohol, and the filtrate treated with an alcoholic solution of mercuric chloride. The ptomaïne $C_7H_{17}NO_2$ is obtained as an easily deliquescent hydrochloride from this precipitate after removal of the mercury by hydrogen sulphide. For purification it is converted into the difficultly soluble aurochloride.

Typhotoxine is isomeric with the compound $C_7H_{17}NO_2$, which Brieger obtained from putrefying horseflesh. Its properties, however, are very different.

The following chemical properties, which distinguish typhotoxine from all other alkaloids, have been noted by Luff[3] and referred to by Gasser.[4] It crystallizes as a white powder. Its hypochlorate, in solution, gives a white precipitate with phosphomolybdic acid; with picric acid and gold chloride, a yellow precipitate; with sodine solution, a dark-colored precipitate; with tannic acid, a dark yellow precipitate. It gives no reaction with phosphowolframic acid, or with platinic chloride.

[1] Weitere Untersuchungen über Ptomaïne, 1885.
[2] Ptomaïnes and Leucomaïnes, 1888.
[3] British Medical Journal, July 27, 1889.
[4] *Loc. cit.*

Its physiological action has been studied on mice and guinea pigs.[1] It produces at first slight salivation, increased respiration, and a paralytic condition of the muscles; salivation increases; heart action and respiration decrease, and the animal dies in from one to two days. Diarrhœic evacuations are frequent throughout, but at no time are convulsions present as in the case of the isomer extracted from putrefying horseflesh.

In conclusion, it is to be said that, at the present time, no single characteristic of the typhoid fever bacillus, other than its ptomaïne, is known which is not possessed by at least one other bacillus; but by taking all of its characteristics together, an expert bacteriologist can obtain a very clear picture of it. During the first few years following its discovery, its characteristics were sufficient to differentiate it from all other bacteria then known; but, with the rapid progress in bacteriology and the more careful study of the water and sewage microörganisms, each of its characteristics was found to be possessed by other species.

The exact state of our knowledge of the typhoid fever bacillus has been well expressed by Koch[2]: "When the typhoid fever bacillus is found in the mesenteric glands, spleen, or liver of a typhoid fever victim, there can be no doubt that one is dealing with the genuine typhoid fever bacillus; for other bacteria have never been found in these places which could be confused with it. But the conditions are wholly changed if it is attempted to identify the typhoid fever bacillus in fæces, soil, water, and dust. For there are found here great numbers of very similar bacteria which only a very skilled bacteriologist can distinguish from the typhoid fever bacillus; and, even then, not with absolute certainty, since unmistakable and constant characteristics are as yet lacking. The numerous statements recently made, that the typhoid fever bacillus has been found in soil, drinking water, and foods, can therefore be received only with a reasonable doubt."

LAWRENCE EXPERIMENT STATION,
MASSACHUSETTS STATE BOARD OF HEALTH,
June 1, 1891.

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[1] Vaughan and Novey, *loc. cit.*

[2] Ueber bakteriologische Forschung. Vortrag. Internat. Med. Congress, August, 1890.

ON THE CANDLE-POWER OF INCANDESCENT LAMPS AS RELATED TO CURRENT, VOLTAGE, AND ENERGY CONSUMED.[1]

By LOUIS A. FERGUSON and DAVID A. CENTER.

The observations described in the present paper have for their object the study of the relation between the illuminating power of various incandescent lamps now in use, and the current, potential, and expenditure of energy used in operating them. The observations were made in the spring of 1888, but various circumstances have delayed their publication till the present time.

The subject has already been considered by several persons, and it has appeared that for such lamps as have been tested the candle-power is approximately proportional to the cube of the current, and to the fifth power of the potential difference between the terminals of the lamp. It seemed to us, however, that it was desirable to carry out a series of tests with more recent lamps, and with a greater variety of them than had been employed in former experiments upon the subject. It was also thought that a consideration of the relation between the candle-power and the potential difference between the terminals of the lamp (or electrical pressure) would be useful.

We considered it best, on the whole, to express our results in the form of a simple exponential equation of the form $y = ax^n$, as has been customary, notwithstanding the fact that, as Götz has shown,[2] at least for the relation between candle-power and energy expended, the results of experiments are more accurately expressed by an equation of the form $y = ax + bx^n$.

The greater simplicity of the common formula has decided advantages in its practical application which go far to make up for

[1] Read at a meeting of the American Academy of Arts and Sciences, June 11, 1890.

[2] Centralblatt für Elektrotechnik, v. 5, p. 720.

the somewhat diminished accuracy at extreme values of the candle-power, while within quite wide ranges it is amply exact for all practical purposes.

There were tested in all twenty-one lamps, including those made by the Edison, United States, Thomson-Houston, and Westinghouse companies, having in every case a good normal filament. The photometric measurements were made with a Bunsen photometer. The lamp to be tested was placed in the circuit of an Edison or Westinghouse direct current dynamo, from either of which machines an electromotive force of 110 volts could be obtained, and by coupling the two machines in series the lamp could be put into a circuit having a potential of 220 volts.

In the same circuit with the lamp to be tested was placed a Thomson graded current galvanometer, by which the current could be measured with ease within one per cent. The difference of potential at the terminals of the lamp was measured by a Thomson potential galvanometer, whose readings also could be depended upon to at least one per cent. The product obtained by multiplying the current in ampères by the difference of potential at the terminals of the lamp in volts, gave the rate in watts at which energy was consumed in the lamp at any given time.

The necessary variation in the current and electromotive force in the working circuit was accomplished by means of a set of six Edison lamps placed in the circuit, with connections between the lamps so arranged that any number of lamps could be thrown out of circuit at a time, or two or three thrown in parallel with each other, thus giving a wide range of variation of resistance. For smaller variations a set of German silver resistance coils was constructed, and arranged so that changes from one fourth of an ohm up to 75 ohms could readily be secured. A Methven slit was used as a standard of light.

In all cases except with those of the United States Company, the lamps were placed so that the plane of the filament was at right angles to the plane of the bar. They were kept in this position throughout the test. The United States lamps were placed so that the plane of the filament made an angle of 45° with the bar.

The lamps were first started at about two tenths of a candle-power, the resistance in circuit then being decreased until a suitable reading was obtained on the photometer, when a series of

twelve or more readings was made of the candle-power by one observer, while the other, as often as possible during the interval, read the current in the circuit and the potential at the terminals of the lamp. The resistance was again changed and new measurements taken of the candle-power, current, and potential, this mode of procedure being continued in most cases until the filament gave way. In some cases it was not broken in the first test, and a second run on the lamps was made. In every such instance it was found that the curve of relation ran lower than in the first test, showing that the effect upon the lamp was to decrease its efficiency, more energy per candle being consumed in the second run. It was also found, in a few instances where it was impossible to finish a test of the lamp on the same day that it was begun, that on resuming the test it required more energy per candle than on the first day, until after some fifteen minutes had elapsed, when the original values were again obtained. This is clearly seen in the Thomson-Houston 32 candle-power, 52 volt lamp denoted in the tables by the letter M.

As a matter of some interest, several tests were made upon some of the Edison lamps to find out, if possible, about how much energy must be expended in such a lamp in order that the first appearance of light might be distinguished with the naked eye in a positively dark room. It was also thought to be of interest to know the color of the light in this case. Measurements were made of the current and potential, and it was found that the mean value of the energy expended when the light first became visible to the most sensitive observer was about 0.74 watts. The color of the light emitted by the filament was in all cases of an ashy gray.

Nearly every style of lamp seemed to have one particular place at which the filament gave way. The Edison filament, in every instance but one, broke at from one eighth to one half an inch above the platinum joint on the right hand side, looking at the labelled side of the globe. The circuit wires were removed from the lamp-socket before this fact was noted, so that we are unable to state whether this was the positive or negative side. The filament of the Weston lamps always broke at about one inch from the top, while that of the Thomson-Houston lamps broke at the top. One of the Westinghouse lamps broke at about one eighth of an inch above the middle point of the right-hand side of the filament, looking

at the labelled side of the globe; another broke at about one eighth of an inch above the joint on the same side. In nearly all the lamps tested the joints were noticed to be very black, due to deposition of carbon by the Crookes' effect.

A series of curves was constructed showing graphically the relations between the candle-power and the current, potential, and energy expended, and equations of the form $y = ax^n$ satisfying the observed values of the variables were determined by the method of least squares for the various lamps in the case of the current and energy determinations. In these the candle-power is denoted by y and the other variable by x. From these equations others, giving the relation between the potential and candle-power, were calculated. This calculation, together with other aid in the preparation of some of our tables, we owe to the kindness of Mr. Edward Collins, Jr., of the Rogers Laboratory.

We had observed that in nearly all cases as the lamp approached the breaking-point the curve ceased to run up with its usual steepness, the candle-power falling off very rapidly. In the calculations of the equation for each lamp, all observations above this point at which the curve fell off were neglected as being of no value for our determinations. This we felt justified in doing, since our object was to deduce an equation which would show the relation between the candle-power and current, or other variable, as long as the former increases regularly with the latter.

In the tables appended the lamps used are designated alphabetically. We calculated the equation for each separate lamp, and from these deduced, by the usual method, a general equation applying to each particular kind of lamp. Below are given the individual equations, as well as the general equations calculated from them.

The Edison lamps used were of the "new style," 3.1 watts per candle-power, and the Weston lamps were the "tamadine lamps."

EDISON, 16 C.P., 110 V.

(A) $y = 500\, x^{5.3}$
(B) $y = 540\, x^{5.0}$
GENERAL EQUATION $v = 520\, x^{5.1}$

EDISON, 10 C.P., 110 V.

(C) $y = 490\, x^{4.7}$

WESTON, 16 C.P., 110 V.

(D) $y = 154\ x^{5.0}$
(E) $y = 160\ x^{4.9}$
GENERAL EQUATION $y = 157\ x^{4.95}$

WESTINGHOUSE, 16 C.P., 50 V.

(F) $y = 7.7\ x^{5.8}$
(G) $y = 6.7\ x^{5.4}$
GENERAL EQUATION $y = 7.21\ x^{5.6}$

THOMSON-HOUSTON, 16 C.P., 75 V.

(H) $y = 71.5\ x^{6.2}$
(J) $y = 68.9\ x^{5.6}$
GENERAL EQUATION $y = 74\ x^{5.9}$

THOMSON-HOUSTON, 16 C.P., 60 V.

(K) $y = 124\ x^{5.48}$
(L) $y = 124\ x^{5.42}$
GENERAL EQUATION $y = 124\ x^{5.45}$

THOMSON-HOUSTON, 32 C.P., 52 V.

(M) $y = .258\ x^{5.9}$
(N) $y = .310\ x^{5.5}$
GENERAL EQUATION $y = .284\ x^{5.7}$

THOMSON-HOUSTON, 20 C.P., 110 V.

(O) $y = 67.5\ x^{4.8}$
(P) $y = 91.0\ x^{5.2}$
GENERAL EQUATION $y = 70.2\ x^{5}$

In like manner were obtained the relations between the candle-power and the energy expended, which are as follows:

EDISON, 16 C.P., 110 V.

(A) $y = 0.000065\ x^{3.1}$
(B) $y = 0.000131\ x^{2.9}$
GENERAL EQUATION $y = 0.000098\ x^{3}$

EDISON, 10 C.P., 110 V.

(C) $y = 0.000087\ x^{3}$

WESTON, 16 C.P., 110 V.

(D) $y = 0.000069\, x^{2.9}$
(E) $y = 0.000051\, x^{3}$
GENERAL EQUATION $y = 0.000059\, x^{2.9}$

WESTINGHOUSE, 16 C.P., 50 V.

(F) $y = 0.00016\, x^{2.6}$
(G) $y = 0.00026\, x^{2.7}$
GENERAL EQUATION $y = 0.000208\, x^{2.7}$

THOMSON-HOUSTON, 16 C.P., 75 V.

(H) $y = 0.000157\, x^{2.8}$
(J) $y = 0.000153\, x^{2.8}$
GENERAL EQUATION $y = 0.000155\, x^{2.8}$

THOMSON-HOUSTON, 16 C.P., 60 V.

(K) $y = 0.00062\, x^{2.7}$
(L) $y = 0.00015\, x^{3.1}$
GENERAL EQUATION $y = 0.00031\, x^{2.9}$

THOMSON-HOUSTON, 32 C.P., 52 V.

(M) $y = 0.000020\, x^{3}$
(N) $y = 0.000098\, x^{2.6}$
GENERAL EQUATION $y = 0.000059\, x^{2.8}$

THOMSON-HOUSTON, 20 C.P., 110 V.

(O) $y = 0.00016\, x^{2.6}$
(P) $y = 0.000059\, x^{2.9}$
GENERAL EQUATION $y = 0.00011\, x^{2.7}$

The following equations showing the relation between the candle-power and potential difference between the lamp terminals, as already stated, were calculated from the preceding equations, except in the case of (F) (G) (P), which were obtained directly from the observed results.

EDISON, 16 C.P., 110 V.

(A) $y = 0.000\,000\,000\,000\,021\, x^{7.3}$
(B) $y = 0.000\,000\,000\,000\,151\, x^{6.8}$
GENERAL EQUATION $y = 0.000\,000\,000\,000\,0082\, x^{7.4}$

EDISON, 10 C.P., 110 V.

(C) $y = 0.000\,000\,000\,000\,000\,18\, x^{8.2}$

WESTON, 16 C.P., 110 V.

(D) $y = 0.000\,000\,000\,000\,11\ x^{6.9}$
(E) $y = 0.000\,000\,000\,000\,01\ x^{7.4}$
GENERAL EQUATION $y = 0.000\,000\,000\,000\,033\ x^{7.1}$

WESTINGHOUSE, 16 C.P., 50 V.

(F) $y = 0.000\,000\,0036\ x^{5.6}$
(G) $y = 0.000\,000\,0047\ x^{5.5}$
GENERAL EQUATION $y = 0.000\,000\,0041\ x^{5.6}$

THOMSON-HOUSTON, 16 C.P., 75 V.

(H) $y = 0.000\,000\,0037\ x^{5.1}$
(J) $y = 0.000\,000\,000\,3\ x^{5.7}$
GENERAL EQUATION $y = 0.000\,000\,0011\ x^{5.4}$

THOMSON-HOUSTON, 16 C.P., 60 V.

(K) $y = 0.000\,000\,0034\ x^{5.4}$
(L) $y = 0.000\,000\,000\,1\ x^{7.4}$
GENERAL EQUATION $y = 0.000\,000\,000\,065\ x^{6.4}$

THOMSON-HOUSTON, 32 C.P., 52 V.

(M) $y = 0.000\,000\,001\ x^{5.9}$
(N) $y = 0.000\,000\,059\ x^{5.0}$
GENERAL EQUATION $y = 0.000\,000\,0089\ x^{5.5}$

THOMSON-HOUSTON, 20 C.P., 110 V.

(O) $y = 0.000\,000\,000\,038\ x^{5.7}$
(P) $y = 0.000\,000\,000\,000\,01\ x^{7.5}$
GENERAL EQUATION $y = 0.000\,000\,000\,000\,62\ x^{6.6}$

The following tables give the data from which the various preceding computations have been derived. They also indicate the deviations of results calculated by the individual formulas from the observed values. The current, potential differences, and energy expended are given in the first three columns headed *C.*, *P. D.*, *E.*, respectively. The fourth column contains the observed candle-power. The remaining columns indicate the candle-power calculated from the current, potential difference, and energy, respectively, together with the deviation of the calculated from the observed values.

EDISON LAMP. 16 C.P. 110 VOLTS. (A)

C.	P. D.	E.	C.P. OBSERVED.	C.P. CALCULATED. (A)	DIFF.	C.P. CALCULATED. (B)	DIFF.	C.P. CALCULATED. (C)	DIFF.
.245	62.52	15.32	.20	.30	—.10	.24	—.04	.27	—.07
.271	69.02	18.70	.46						
.304	75.19	22.86	.97	.93	.03	.93	.04	.93	.04
.348	84.14	29.28	2.23						
.374	88.06	32.93	3.28						
.425	97.52	41.45	6.56	5.96	.60	6.13	.42	5.76	.80
.468	105.60	49.42	10.74						
.479	107.43	51.46	12.10						
.494	109.96	54.32	15.19						
.568	117.34	66.65	24.96	25.29	—.33	23.56	1.40	24.60	.36
.598	120.95	72.33	31.69						
.638	126.33	80.60	42.06	46.69	—4.63	40.35	1.71	44.03	—1.97
.683	136.02	92.90	60.25						
.703	139.27	97.91	69.71	78.00	—8.29	81.96	—12.25	74.43	—4.72
.749	145.47	108.96	87.58						
.824	158.12	130.29	113.20	180.50	—67.30	112.14	1.06	191.50	—78.30

EDISON LAMP. 16 C.P. 110 VOLTS. (B)

C.	P. D.	E.	C.P. OBSERVED.	C.P. CALCULATED. (A	DIFF.	C.P. CALCULATED. (B)	DIFF.	C.P. CALCULATED. (C)	DIFF.
.235	65.05	15.29	.295	.371	—.076	.311	—.016	.356	—.061
.262	71.10	18.63	.563						
.294	76.40	22.46	1.09	1.15	—.06	.93	.16	1.09	0
.320	82.59	26.43	1.79						
.349	88.70	30.97	2.93						
.372	92.65	34.47	4.28	3.74	.54	3.72	.02	3.76	.52
.411	100.53	41.32	7.26						
.445	107.21	47.71	11.02						
.490	116.16	56.92	16.40	16.57	—.17	15.97	.60	16.10	.30
.573	129.17	74.01	30.60						
.607	134.94	81.91	41.69						
.658	143.56	94.46	61.72	65.90	—4.18	67.31	—1.41	69.91	—8.19
.721	156.37	112.74	88.27						

Edison Lamp. 10 C.P. 110 Volts.

C.	P. D.	E.	C.P. Observed.	C.P. Calculated. (A)	Diff.	C.P. Calculated. (B)	Diff.	C.P. Calculated. (C)	Diff.
.202	64.49	13.03	.14	.26	—.12	.10	.04	.18	—.04
.293	84.82	24.85	1.37						
.341	94.81	32.33	3.08	3.10	.31	2.30	.78	2.78	.30
.395	107.85	42.60	7.97						
.433	114.84	49.73	12.22	9.54	2.68	10.93	1.29	10.03	2.19
.507	130.13	65.98	24.17	20.05	4.12	30.35	—6.18	23.33	.84
.616	148.00	91.17	54.59						
.678	158.02	107.14	78.02	78.81	—.79	147.40	—69.40	92.18	—14.16

WESTON LAMP. 16 C.P. 110 VOLTS. (D)

C.	P. D.	E.	C.P. OBSERVED.	C.P. CALCULATED. (A)	DIFF.	C.P. CALCULATED. (B)	DIFF.	C.P. CALCULATED. (C)	DIFF.
.317	68.63	21.76	.420						
.365	76.65	27.98	.695	1.034	—.339	1.092	—.397	1.014	—.319
.409	83.86	34.30	1.66						
.458	91.24	41.19	3.57	3.21	.36	3.63	—.06	3.09	.48
.482	94.64	45.62	4.74						
.539	103.29	55.67	8.06	7.18	.88	8.54	—.48	7.36	.70
.578	108.92	62.96	11.39						
.630	115.96	73.05	18.31	15.56	2.75	18.93	—.62	16.08	2.23
.735	129.29	95.03	35.24						
.801	135.72	108.71	51.61	51.08	.53	55.95	—4.34	50.54	1.07
.877	141.21	123.84	70.47						
.977	155.40	151.83	113.69	137.22	—23.53	142.32	—28.63	132.30	—18.61

WESTON LAMP. 16 C.P. 110 VOLTS. (E)

C.	P. D.	E.	C.P. OBSERVED.	C.P. CALCULATED. (A)	DIFF.	C.P. CALCULATED. (B)	DIFF.	C.P. CALCULATED. (C)	DIFF.
.327	70.74	23.13	.465						
.362	76.54	27.71	.863	1.078	—.215	.885	—.022	.951	—.088
.387	81.66	31.60	1.53						
.416	86.03	35.79	2.12	2.12	0	2.10	.02	2.03	.09
.471	94.34	44.43	4.18						
.530	102.78	54.47	7.65	6.97	.68	7.85	—.20	7.04	.61
.593	112.30	66.59	13.23						
.629	117.51	73.92	18.72	16.24	2.48	21.13	—2.59	17.36	1.36
.755	132.28	99.94	44.16						
.800	137.48	109.98	58.33	53.28	5.05	67.54	—9.21	56.30	2.03
.899	148.32	133.34	98.23						
.987	157.68	155.94	133.72	151.87	—18.15	237.40	—103.68	158.06	—24.34
1.037	166.08	172.23	144.92						
1.071	175.60	188.07	155.93						

WESTINGHOUSE LAMP. 16 C.P. 50 VOLTS. (F)

C.	P. D.	E.	C.P. Observed.	C.P. Calculated. (A)	Diff.	C.P. Calculated. (B)	Diff.	C.P. Calculated. (C)	Diff.
.627	28.58	17.92	.426	.509	—.083	.542	—.116	.543	—.117
.684	31.12	21.29	.761						
.803	36.33	29.17	2.22	2.16	.04	2.09	.13	2.16	.04
.915	41.54	38.01	4.98						
.979	44.77	43.83	7.98	6.83	1.15	6.73	1.25	6.85	1.13
1.089	50.12	54.58	14.74						
1.180	53.79	63.37	22.00	20.07	1.93	18.86	3.14	19.43	2.57
1.360	62.74	85.58	49.65						
1.440	66.74	96.11	67.50	64.57	2.93	63.36	4.14	63.14	4.36
1.560	73.07	113.80	98.43						
1.610	75.55	121.60	114.20	123.50	—9.30	127.00	—12.80	122.80	—8.60
1.700	80.76	137.50	141.50						

WESTINGHOUSE LAMP. 16 C.P. 50 VOLTS. (G)

C.	P. D.	E.	C.P. OBSERVED.	C.P. CALCULATED. (A)	DIFF.	C.P. CALCULATED. (B)	DIFF.	C.P. CALCULATED. (C)	DIFF.
.679	31.48	21.37	.744	.831	—.087	.780	—.036	.874	—.130
.849	38.94	33.06	2.78						
.941	42.66	40.14	4.76	4.84	—.08	4.13	.63	4.75	.01
1.040	47.87	49.78	8.90						
1.150	53.36	61.36	16.18	14.13	2.15	14.11	2.17	14.29	1.89
1.320	61.19	80.77	30.76						
1.500	69.98	105.00	59.94	59.92	.02	62.53	—2.59	59.32	.62
1.500	73.08	113.30	74.46						
1.710	80.53	137.70	115.80	123.10	—7.30	135.16	—29.36	121.65	—5.85
1.790	84.90	152.00	141.80	153.80	—12.00	180.50	—38.70	158.40	—16.60
1.930	95.60	185.60	172.40						

THOMSON–HOUSTON LAMP. 16 C.P. 75 VOLTS. (H)

C.	P. D.	E.	C.P. OBSERVED.	C.P. CALCULATED. (A)	DIFF.	C.P. CALCULATED. (B)	DIFF.	C.P. CALCULATED. (C)	DIFF.
.414	42.80	17.72	.389	.302	.087	.693	—.304	.351	.038
.460	48.29	22.21	.855	.581	.274	1.280	—.425	.882	—.027
.540	54.91	29.65	2.09	1.568	.52	2.40	—.31	2.07	.02
.616	61.90	38.13	4.53	3.55	.98	4.51	.02	4.19	.34
.672	67.87	45.61	7.79	6.09	1.70	7.19	.60	6.92	.87
.746	73.86	55.10	13.22	12.45	.77	11.04	2.18	11.75	1.47
.817	79.84	65.18	20.43	20.42	.01	16.38	4.05	18.40	2.03
.938	91.80	86.11	41.94	48.10	—6.16	33.25	8.69	41.03	.91
.982	94.56	92.86	49.89	63.89	—14.00	38.64	11.25	50.69	—.80
1.10	105.60	116.20	79.50	129.20	—49.80	67.67	11.83	95.01	—15.51
1.17	112.40	131.50	96.03	189.36	—93.33	92.87	3.16	134.30	—38.27

THOMSON–HOUSTON LAMP. 16 C.P. 75 VOLTS. (J)

C.	P. D.	E.	C.P. OBSERVED.	C.P. CALCULATED. (A)	DIFF.	C.P. CALCULATED. (B)	DIFF.	C.P CALCULATED. (C)	DIFF
.441	45.06	19.87	.607	.680	—.073	.763	—.156	.719	—.112
.488	49.28	24.05	1.17	1.20	—.03	1.27	—.10	1.24	—.07
.550	53.50	28.36	1.90	1.92	—.02	2.02	—.12	1.97	—.07
.562	56.32	31.65	2.66	2.67	—.01	2.71	—.05	2.69	—.03
.631	63.02	39.77	5.58	5.13	.45	5.13	.45	5.13	.45
.677	67.44	45.66	8.26	7.63	.63	7.51	.75	7.58	.68
.753	74.43	56.04	15.09	13.91	1.19	13.18	1.91	13.54	1.55
.843	83.07	70.03	26.64	26.27	.34	24.70	1.94	25.42	2.22
.904	89.55	80.95	40.46	39.00	1.46	37.72	2.74	38.34	2.12
.975	96.87	94.45	58.83	59.70	—.87	58.93	—.10	59.32	—.49
1.120	110.20	123.40	113.69	130.50	—16.81	122.58	—8.89	126.41	—12.72
1.220	125.30	152.90	138.50						

THOMSON-HOUSTON LAMP. 16 C.P. 60 VOLTS. (K)

C.	P. D.	E.	C.P. OBSERVED.	C.P. CALCULATED. (A)	DIFF.	C.P. CALCULATED. (B)	DIFF.	C.P. CALCULATED. (C)	DIFF.
.349	30.64	10.70	.323	.308	.015	.413	—.090	.401	—.078
.428	37.31	15.97	1.175						
.490	42.72	20.93	2.770	2.484	.286	2.505	.265	2.503	.267
.572	49.99	28.59	6.520						
.640	56.04	35.87	11.93	10.77	1.16	11.02	.91	10.90	1.03
.718	63.13	45.33	22.16						
.797	70.68	56.33	39.22	35.80	3.42	38.95	.27	37.33	1.89
.872	76.82	64.99	53.84						
.931	83.92	78.13	67.50	83.91	—16.41	99.15	—31.65	91.21	—23.71

THOMSON–HOUSTON LAMP. 16 C.P. 60 VOLTS. (L)

C.	P. D.	E.	C.P. OBSERVED.	C.P. CALCULATED. (A)	DIFF.	C.P CALCULATED. (B)	DIFF.	C P. CALCULATED. (C)	DIFF.
.381	33.03	12.58	.514	.664	—.150	2.85	.229	.418	.096
.420	36.55	15.35	1.04						
.461	40.41	18.63	1.85	1.86	—.01	1.29	.56	1.42	.43
.490	43.03	21.08	2.71						
.526	46.04	24.22	4.21	4.21	0.0	2.69	1.52	3.24	.97
.588	52.10	30.63	8.14						
.662	59.28	39.24	16.50	13.27	3.23	16.44	.06	14.67	1.83
.724	64.71	46.85	23.71						
.807	72.60	58.59	44.57	38.81	5.76	75.51	—30.94	51.43	—6.86
.887	80.59	71.78	70.47						
.957	90.53	86.64	92.02	97.75	—5.73	387.60	—295.58	175.00	—82.98

THOMSON–HOUSTON LAMP. 32 C.P. 52 VOLTS. (M)

C.	P. D.	E.	C.P. OBSERVED.	C.P. CALCULATED. (A)	DIFF.	C.P. CALCULATED. (B)	DIFF.	C.P. CALCULATED. (C)	DIFF.
1.10	26.61	29.27	.366						
1.32	31.76	41.92	1.30	1.62	—.32	1.28	.02	1.34	—.04
1.44	34.45	49.61	2.25						
1.61	38.58	62.11	4.98	5.24	—.26	3.53	1.45	4.30	.68
1.84	43.79	80.57	10.55						
1.98	47.73	94.11	17.03	17.93	—.90	12.50	4.53	18.60	—.67
2.14	51.48	110.20	26.10						
2.31	55.84	129.00	40.04	44.76	—4.72	31.78	8.26	37.71	2.33
2.46	59.42	146.20	56.97						
2.67	64.63	172.60	86.21	105.80	—19.59	75.63	11.58	89.52	—3.31
2.83	68.15	192.90	109.60						
2.98	72.17	215.10	139.20	203.17	—63.97	145.82	—6.62	172.10	—32.90
3.01	71.89	216.40	117.50						
3.10	74.29	230.30	132.00						
3.27	78.71	257.40	155.10						
3.38	81.66	276.00	155.10						
3.76	93.07	349.90	191.20						

THOMSON-HOUSTON LAMP. 32 C.P. 52 VOLTS. (N)

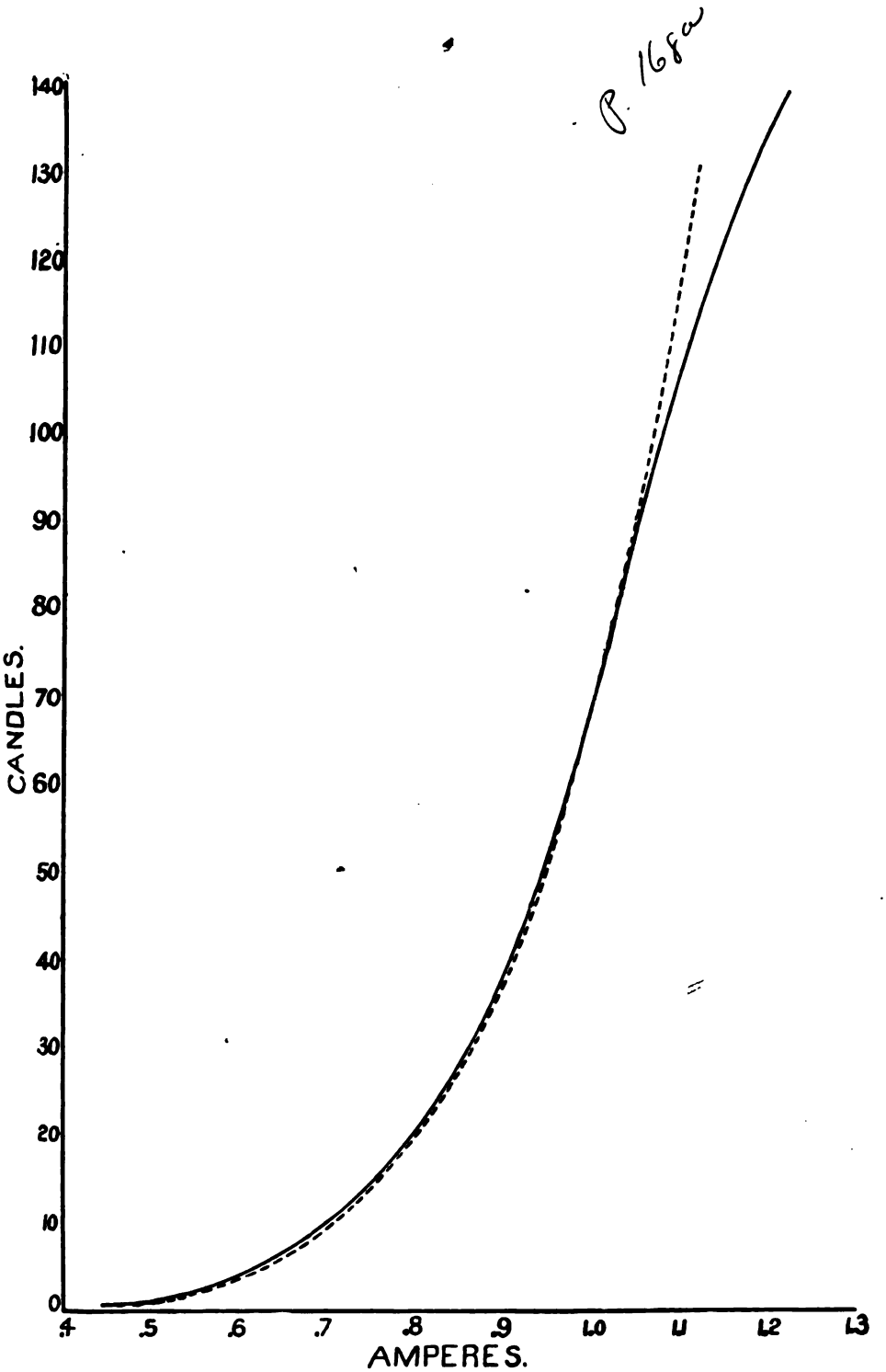
C.	P. D.	E.	C.P. OBSERVED.	C.P. CALCULATED. (A)	DIFF.	C.P. CALCULATED (B)	DIFF.	C.P. CALCULATED. (C)	DIFF.
1.22	28.39	34.64	.730						
1.40	32.38	45.33	1.76	1.95	—.19	2.32	—.56	2.14	—.38
1.55	35.85	55.57	3.60						
1.71	39.42	67.40	6.35	5.80	.55	6.23	.12	6.04	.31
1.81	41.90	75.84	9.08						
1.94	45.06	87.42	13.52	11.64	1.88	12.21	1.41	11.94	1.58
2.10	48.72	102.30	20.34						
2.30	53.56	123.20	35.01	29.54	5.47	29.11	5.90	29.34	5.67
2.47	57.87	142.90	48.00						
2.64	62.23	164.30	72.62	62.81	9.81	61.93	10.69	62.36	10.26
2.82	66.54	187.60	95.05						
3.08	72.37	222.90	151.30	145.91	5.39	131.96	19.34	138.68	12.62
3.49	82.84	287.10	246.70						
3.79	90.31	342.30	307.50	453.78	—146.28	402.63	—95.13	426.68	—119.18

THOMSON–HOUSTON LAMP. 20 C.P. 110 VOLTS. (O)

C.	P. D.	E.	C.P. OBSERVED.	C.P. CALCULATED. (A)	DIFF.	C.P. CALCULATED (B)	DIFF	C.P. CALCULATED. (C)	DIFF.
.412	67.05	27.62	.633						
.437	70.63	30.87	.945	1.251	—.306	.886	—.059	1.195	—.250
.469	75.55	35.43	1.57						
.509	82.09	41.78	2.79	2.63	.16	2.62	.17	2.62	.17
.563	89.41	50.34	5.07						
.632	96.87	61.22	8.70	7.45	1.25	6.68	2.02	7.07	1.63
.660	101.90	67 24	12.38						
.727	111.50	81.07	20.31	14.55	5.76	14.80	5.51	14.68	5.63
.784	117.80	92.39	28.38						
.883	131.40	116.00	39.76	37.08	2.68	37.50	2.26	37.25	2.51
.959	140.40	134.60	56.04						
1.065	152 30	162.20	84.37	91.39	—7.02	86.46	—2.09	89.00	—5.63
1.140	162.10	185.30	107.40						
1.230	173.30	213.90	128.70	185.50	—56.80	179.56	—50.86	182.77	
1.260	186.40	234.90	117.00						—54.07

Thomson Houston Lamp. 50 C.P. 110 Volts (P)

C.	P.D.	E.	C.P. Observed	C.P. Calculated (A)	Diff.	C.P. Calculated (B)	Diff.	C.P. Calculated (C)	Diff.
.374	64.63	[illegible]	.449	.557	—.108	.823	.156	.949	.100
.425	71.53	30.40	1.07						
.464	77.44	35.93	1.67	1.70	—.03	1.58	.43	1.71	.04
.543	88.00	47.78	4.30						
.605	96.59	58.41	7.69	6.72	.97	6.49	1.20	6.91	.78
.664	105.00	69.71	12.73						
.748	114.80	85.90	22.03	20.16	1.87	21.55	.482	20.91	1.13
.867	129.20	112.10	43.49						
.959	137.00	131.40	64.54	72.96	—8.42	88.10	21.56	70.84	6.30
1.020	146.40	149.80	93.91						
1.110	156.80	173.80	146.00	155.00	—9.00	241.60	—94.60	158.00	14.00


B. 168a
140
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CANDLES.
.4
.5
.6
.7
.8
.9
1.0
1.1
1.2
1.3
AMPERES.

Fig. 1.

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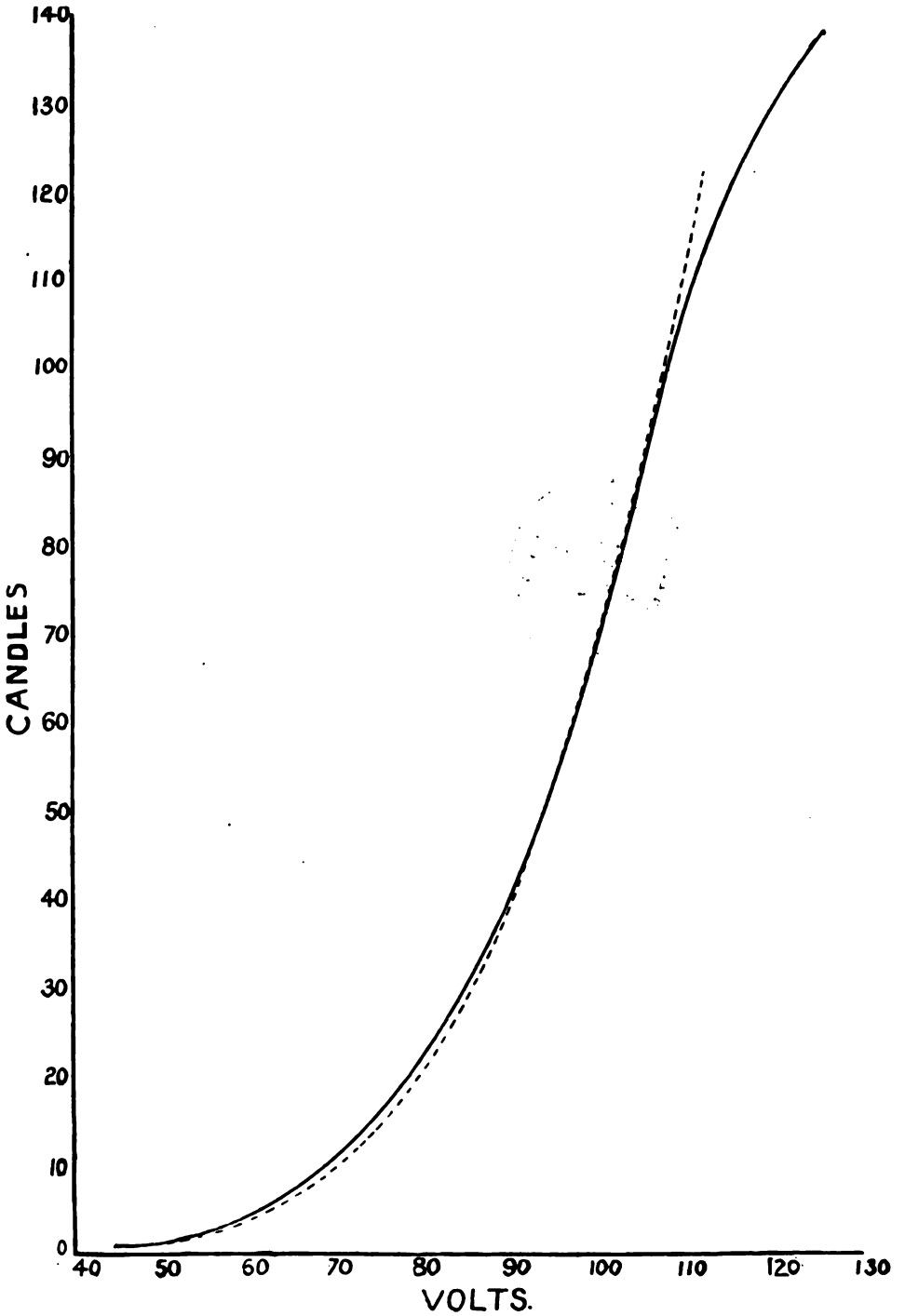
140
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40
50
60
70
80
90
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110
120
130
VOLTS.

Fig. 2.

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Although our general equations would be much enhanced in value if we could have employed a larger number of each kind of lamp in our investigations, there are, nevertheless, certain facts that are clearly evident from our results. The lamps at present used, like the earlier ones, increase in candle-power about as the fifth power of the current employed, and as the cube of the energy expended. It will also be seen that the candle-power increases about as the sixth power of the potential difference between the terminals of the lamp. The average value of the exponent of x, the candle-power for the current equations, is 5.3; for the energy equations, 2.9; and for the potential difference equations, 6.4.

To illustrate the agreement of the exponential equations with the observed relations between candle-power and current, potential difference, and energy expended, the curves shown in Figs. 1, 2, 3, were plotted for the Thomson-Houston 16 C.P., 75 volt lamp, indicated by the letter J in the tables. The equations used were the individual ones for that special lamp.

ROGERS LABORATORY OF PHYSICS.
June, 1890.

THE GEOMETRICAL ISOMERISM OF THE CHLORCINNAMIC ACIDS.[1]

By S. P. MULLIKEN, Ph.D.

Previous to the recent publication of A. Michael on the chlorcinnamic acids, which appeared in the *Journal für praktische Chemie*, **40**, 63, shortly before the completion of the author's work on the same subject in the laboratory of Wislicenus, our knowledge of the isomeric relations of these acids was extremely incomplete and confused. The two acids which should properly be designated as *α* chlorcinnamic acid and *α* chlorisocinnamic acid were first obtained by Jutz[2] in 1882, by treating cinnamic acid dichloride with alchoholic potash. Jutz adopted the same nomenclature that had been employed by Glaser[3] for the corresponding bromcinnamic acids, and called the acid with the melting point 142° *α* chlorcinnamic acid, and that with the melting point 114° *β* chlorcinnamic acid. The correctness of this nomenclature in the case of the *α* acid was soon after shown by Plöchl,[4] who succeeded in preparing it by Perkin's reaction from benzaldehyde and sodium chloracetate. According to Plöchl, the so-called *β* acid forms scarcely any of its isomer on being repeatedly distilled, or even after being heated with hydriodic acid — a behavior which is somewhat surprising when the easy transformation of the *β* bromcinnamic acid of Glaser into his *α* bromcinnamic acid is recalled. Still another proof that the chlorcinnamic acid of higher melting point has its Cl atom in the *α* position, is given by Forrer,[5] who obtained a chlorcinnamic acid of melting point 138° –

[1] From an "Inaugural Dissertation" "Ueber die Konstitution der Chlorzimmtsäuren," Leipzig, 1890.

[2] Jutz, Berichte der deutschen chemischen Gesellschaft, 15, 788.

[3] Glaser, Liebig's Annalen, 143, 330.

[4] Plöchl, Berichte der deutschen chemischen Gesellschaft, 15, 1945.

[5] Forrer, Berichte der deutschen chemischen Gesellschaft, 16, 854.

139° by withdrawing one molecule of water from phenylchlorlactic acid ($C_6H_5CH.OH.CHCl.COOH$).

About two years later Perkin[1] obtained the first real β chlorcinnamic acid by treating benzoylacetic ether with phosphorus-pentachloride, and decomposing the product of the reaction with water. Perkin, however, erroneously believed his acid to be identical with the *a* acid of Jutz, because both exactly coincided in their melting points. Finally, Michael, in the article already mentioned, describes the preparation of two β chlorcinnamic acids by allowing hydrochloric acid and phenylpropiolic acid in glacial acetic acid solution to react on each other in sealed tubes for about a week at the ordinary temperature. One of these isomers melted at 132.5°; the other at 142°. Michael rightly considers that both are to be regarded as having their Cl atom in the β position; or, in other words, that they are geometrical isomers standing in a similar relationship to each other as the two isomers discovered by Jutz. The experiments of the author confirm the conclusions of Michael, and were further intended to show, by employment of methods already used by Wislicenus, which stereochemical formulæ probably represent the constitution of the several isomers.

The two *a* acids were prepared by the method of Jutz, according to the reactions: —

$$C_6H_5CH.\ CH.\ COOH + Cl_2 = C_6H_5CHCl.\ CHCl.\ COOH, \text{ and}$$
$$C_6H_5CHCl.\ CHCl.\ COOH + 2KOH = C_6H_5CH : CCl.\ COOK + KCl + H_2O.$$

The formation of two geometrical isomers from cinnamic acid dichloride may be ascribed to a series of reactions analogous to that which occurs in the case of the formation of the *a* chlorcrotonic acids from isocrotonic acid dichloride (Wislicenus, Liebig's Annalen, **248**, 286). The isomeric acids were separated by taking advantage of the great difference in the solubility of their potassium salts in strong alcohol, the salt of the acid of lower melting point requiring only 3.58 parts of absolute alcohol for solution at 20°, while the salt of the acid with higher melting point requires 638 parts of this solvent at the same temperature. The pure acids were found to have their respective melting points at 110.5° and 139° — an observation which agrees quite closely with those made by Forrer and Michael, and indicates that the melting points as determined by Jutz were both too high.

[1] Perkin, Journal of Chemical Society, **47**, 256.

In studying the constitutions of these isomers, it had first to be shown that they were actually geometrical and not structural isomers. This could be accomplished by examining their behavior on heating; for, like other double-bonded organic acids which exist in two geometrically isomeric modifications, one of the *a* chlorcinnamic acids contains its constituent atoms and radicals grouped in an arrangement of more stable equilibrium than the other, so that the simple application of heat to the less stable compound is sufficient to convert it into the more stable form. The experiments made to determine the effects of heat were carried out by heating weighed quantities of the pure acids, enclosed in sealed tubes, in a thermostat, then exactly neutralizing the alcoholic solutions of the acids taken from the tubes with an alcoholic solution of potash, and separating, drying and weighing the difficultly and easily soluble potassium salts thus formed. The following tabulation of the results obtained shows that the acid of melting point 139°, even after long heating at 180°, remained quite unchanged; while of the acid melting at 110.5°, more than three fourths were converted into its more stable isomer.

	Temperature.	Duration of heating.	Unchanged acid.	Acid converted into its isomer.
Acid m. pt., 110.5°	160°	5 hours.	—— per cent.	53 per cent.
	176°	5 "	—— "	73 "
	178°	10 "	—— "	90 "
Acid m. pt., 139°	140°	5 "	100.0 "	—— "
	160°	5 "	99.2 "	—— "
	180°	6 "	99.5 "	—— "

The principle that HCl is more readily withdrawn by potash from compounds containing H and Cl attached to two carbon atoms connected by a double bond, when the H and Cl atoms are in plane-symmetrical positions to one another, than when they occupy axially symmetrical positions, has been assumed and frequently employed by Wislicenus in explaining the constitution of geometrical isomers. Applying this method to the *a* chlorcinnamic acids, it is evidently to be expected that of the two configurations,

$$\begin{array}{l} H-C-C_6H_5 \\ \quad\| \\ Cl-C-COOH \\ \quad (I) \end{array} \quad \text{and} \quad \begin{array}{l} C_6H_5-C-H \\ \quad\| \\ Cl-C-COOH, \\ \quad (II) \end{array}$$

(I) would lose HCl and form the triple bonded phenylpropiolic acid $\begin{array}{l} C-C_6H_5 \\ \|\| \\ C-COOH \end{array}$ much more readily than (II).

Aqueous solutions of the potassium salts of both acids were prepared by dissolving in each case 0.5 gm. (1 mol.) of the acid in 10 cc. of water containing 2½ mol. of KOH. These tubes were immersed side by side in a bath of boiling water, and heated, in one experiment for ten, and in a second for thirty minutes. The percentage of chlorine withdrawn as potassium chloride was then gravimetrically determined for each tube, and from these results the percentage of decomposed acid calculated.

Acid	Duration of heating.	Per cent of acid decomposed.
m. pt., 139°	10 minutes.	6.62 per cent.
" 110.5°	10 "	0.99 "
" 139°	30 "	39.02 "
" 110.5°	30 "	2.41 "

From the above it will be seen that the *a* chlorcinnamic acid melting at 139° should have the constitution expressed by (I), while its isomer, melting at 110.5°, should be represented by (II).

In preparing the *β* chlorcinnamic acids, resort was made to two methods. The first consisted in heating phenylpropiolic acid in sealed tubes with an excess of fuming hydrochloric acid for several hours, to a temperature between eighty and ninety degrees.

$$\begin{matrix} C_6H_5-C \\ \| \\ HOOC-C \end{matrix} + HCl = \begin{matrix} C_6H_5-C-Cl \\ | \\ HOOC-C-H \end{matrix}$$

If a much higher temperature be used, the chlorcinnamic acids formed are decomposed, giving rise to acetophenon and carbonic acid. At the ordinary temperature the addition of hydrochloric acid does not take place. The two geometrical isomers produced by the reaction are separated by means of their potassium salts. The potassium salt of the acid melting at 132° is soluble in 1.2 parts of absolute alcohol at 20°; while the potassium salt of the acid melting at 143° dissolves in 386 parts of alcohol at the same temperature. The yield of the acid with the higher melting point was about one and a half times as large as that of its isomer. When it is desired to obtain this acid of *higher melting point only*, Perkin's method is to be recommended, as benzoylacetic ether, at the present time, is more accessible than phenylpropiolic acid. The reactions that take place are: —

$$C_6H_5.CO.CH_2.COOC_2H_5 + 2PCl_5 = C_6H_5.CCl_2.CH_2.COCl + C_2H_5Cl + 2POCl_3,$$

and, $C_6H_5.CCl_2.CH_2.COCl + H_2O = HCl + C_6H_5.CCl : CH.COOH.$

The fact that these two chlorcinnamic acids are geometrical and not structural isomers, was shown by converting one into the other by the agency of heat, in the manner described in speaking of the *α* chlorcinnamic acids. It was found, for instance, after heating a portion of the acid melting at 132° for five hours at 170°, that 76% of the acid had been changed into its isomer melting at 143°. It was, on the contrary, found impossible to detect any of the lower melting acid in the acid of higher melting point after a similar treatment. In this connection it is deserving of remark, that, while the *α* chlorcinnamic acids may be heated for hours at 170° without showing any sign of decomposition, the *β* compounds are much less stable, and after heating are always mixed with more or less tarry matter.

The *β* chlorcinnamic acids can be easily reduced by mild reducing agents; but, unfortunately, a molecular rearrangement appears to accompany the reduction, so that common cinnamic acid was always the only product that could be isolated. When we consider the remarkable tendency, which, as Liebermann[1] has shown, the isocinnamic acid possesses to change in its solutions into the ordinary cinnamic acid, this observation becomes hardly surprising.

As a means of directly proving the *β* position of the Cl atom in the chlorcinnamic acid of melting point 142°, a small quantity of the methyl ether was prepared from its silver salt and methyl iodide. The alcoholic solution of this ether, which formed well developed colorless crystals melting at 25°, was treated cautiously with somewhat more than the calculated quantity of a solution of sodium ethylate. The reaction,

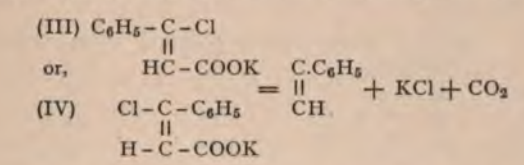
$$C_6H_5CCl : CH.COOC_2H_5 + C_2H_5ONa = NaCl + C_6H_5COC_2H_5 : CH.COOCH_3,$$

which was allowed to take place in the cold, yielded an oil boiling, under a pressure of 77 mm., between 209° and 211°. Analysis showed this oil to have very nearly the percentage composition required for the methyl ether of *β* ethoxylcinnamic acid. When left for several hours in contact with sulphuric acid containing water, at the temperature of the laboratory, the solution of the oil in dilute alcohol gave a strong violet-red coloration on being mixed with ferric chloride solution. The benzoylacetic ether, to which this coloration was due, was, however, destroyed by continued action of

[1] Liebermann, Berichte der deutschen chemischen Gesellschaft, 23, 144.

the sulphuric acid, so that the oil finally consisted largely of acetophenon, as was proved by converting a portion of it into the characteristic compound with phenylhydrazine. The phenylhydrazine compound melted at 105°, and contained exactly the percentage of nitrogen required by the formula $C_{14}H_{14}N_2$.

For the purpose of determining the stereochemical formulæ of the β chlorcinnamic acids, it was found practicable to employ, besides determining the rate at which HCl is split off from the two isomers by potash, another method of the same type, based on the fact, that, like the alkali salts of other organic acids having a Cl atom in the β position, the potassium salts of the β chlorcinnamic acids decompose on being heated in aqueous solution into an unsaturated hydrocarbon, alkaline chloride, and carbon dioxide:—



(III) $C_6H_5-C(Cl)=CH-COOK$

or,

(IV) $Cl-C(C_6H_5)=CH-COOK$

$= C_6H_5-C\equiv CH + KCl + CO_2$

Now it is reasonable to suppose that the compound (III) would undergo such a splitting more readily than (IV), in which the groups to be removed lie on opposite sides of the central axis of the system. The experiments were all made by heating 0.5 gm. of the pure potassium salts, mixed with 2.5 cc. of water, in closed tubes, in a thermostat. The products were always phenylacetylene boiling at 141° – 142°, potassium chloride and carbon dioxide. Silver nitrate precipitated the phenylacetylene from its solution in dilute alcohol as phenylacetylene-silver ($C_6H_5C\equiv CAg$)[1], whose purity was established by a silver determination.

In the quantitative experiments made to ascertain the rate at which the decomposition of the two potassium salts progressed, the chlorine in the potassium chloride formed by the reaction, was determined by precipitation with silver nitrate.

K salt of acid melting at	Duration of heating.	Temperature.	Per cent. of K salt decomposed.
143°	2½ hours.	120°	15.8 per cent.
132.5°	2½ "	120°	76.8 "
143°	5 "	127°	29.9 "
132.5°	5 "	127°	100.0 "

[1] Glaser, by a singular mistake, assigns to phenylacetylene-silver the formula ($C_6H_5C\equiv CAg)_2$ Ag_2O.

The β chlor-cinnamic acid melting at 132°, accordingly, corresponds to configuration (III), and the acid of melting point 143° to configuration (IV).

The experiments next made were to confirm the above results by heating the acids with an excess of potash solution, just as was done in the investigation of the α acids. The salt represented by configuration (III), according to the theory, should split off H and Cl more slowly under such treatment than the salt of configuration (IV); and, as the acid of melting point 143° was found to be the more reactive of the two isomers, the probability that the formula $\begin{matrix} Cl\text{-}C\text{-}C_6H_5 \\ \| \\ H\text{-}C\text{-}COOH \end{matrix}$ is actually the one representing this acid, thus receives fresh support. The numerical results obtained were the following: —

Acid	Duration of heating.	Temperature of both.	Per cent. of acid decomposed.
m. pt., 143°	10 minutes.	100°	100.0 per cent.
" 132.5°	10 "	100°	56.5 "
" 143°	30 "	100°	100.0 "
" 132.5°	30 "	100°	80.5 "

Finally, after having seen how an unusually complete series of reactions all agree in indicating certain definite stereochemical formulæ for each of the chlorcinnamic acids, it remains to be added, that, striking as the cumulative evidence furnished by the agreement of these typical Wislicenian methods is, it is yet hardly sufficient to place the conclusions arrived at in a position secure against attack. Preliminary measurements of the electrical conductivity of the α acids by the author, gave affinity coefficients,[1] which at present seem irreconcilable with the stereochemical constitutions above assigned to these substances, although they completely confirm the conclusions reached regarding the ordinary structural symbols. Similar measurements of the β acids showed much smaller values for K than those possessed by the α acids; but the difference between the conductivity of the two geometrical isomers themselves was too slight to permit of safely drawing conclusions about their geometrical structure.

A like disagreement between the results obtained by the employ-

[1] For α acid, m. pt. 110.5°, K = 1.07. For α acid, m. pt. 139°, K = .097. For β acids, K = .0275 (approximately).

ment of the chemical and the electrical methods has just been brought to light by J. Mendelsohn (Dissertation, Leipzig, 1891), in a study of the β bromcinnamic acids; while the β chlorcrotonic acids furnish still another riddle of the same kind. Do these contradictions show the unfitness of either of these methods for use in solving such problems of stereochemistry? If not, the customary manner of interpreting the data furnished by at least one of them must be at fault. The question is one whose importance will be recognized by all who are interested in stereochemical matters. Its discussion, however, cannot be attempted in this place.

BRYN MAWR COLLEGE,
September, 1891.

THE ORIGIN OF DEATH.

By Edward G. Gardiner, Ph.D.

Among the most brilliant of modern biologists is to be reckoned Prof. Augustus Weismann, of Freiburg. His numerous contributions to science place him in the rank of those whose words are never without interest and value to the naturalist; but, besides his publications, which give record of long and laborious experiments and observation, he has added a series of essays of profound interest, not only to the specialist, but to every mind which takes pleasure in scientific speculation.

These essays are devoted to the discussion of a new theory of heredity; but, besides this, he advances views which to many must seem startling, viz.: that death, in the sense in which we know it, did not always exist, but is something which has been acquired by the higher animals as they were evolved from the lower; and further, that individuals of a large group of animals living today are potentially immortal.

In one of his earlier essays he points out that the manner of reproduction among the Protozoa is such that death does not normally occur in this group; for the animal reproduces by merely dividing itself into halves. Thus an adult animal ceases to exist as such, by becoming two animals instead of one. It does not die during this process, for there is no corpse; but the whole animal, as such, has completely disappeared, and in its place we find two individuals so similar that it is impossible to regard them as parent and offspring. Indeed, they cannot be parent and offspring, for they are of the same generation; it is more natural to call them twins. They are both young animals, for they increase in size; and when adult, each of them ceases to exist by dividing itself into two new young ones, and so on indefinitely.

Hence it would appear that the life history of such an animal may be divided into two periods — youth and adult life. There is no old age; there is no death. Clearly, then, since these forms do not die, they may be said to be potentially immortal. The living matter of which they are composed passes over without break into a younger generation, and in it, life is continued. These facts have been long known, and earlier investigators have pointed out the potential immortality which this mode of reproduction implies; but Weismann was the first to develop this knowledge into a scientific theory which may throw light on other facts.

When Weismann calls these animals immortal, he draws a proper distinction between the terms immortal and eternal. Eternity reaches back into the past, as well as out into the future. With eternity he has nothing to do. Neither does he use immortality in the sense in which it is used in theology — as applied to something which can never die, but must exist through all future time. His claim is not that the life of a Protozoön is such that it must under any circumstances exist forever, but that it will exist as long as proper physical conditions exist; that death is not inherent in life.

He compares the life cycle of a Protozoön to the circulation of water, which evaporates, gathers in clouds, and falls to the earth, only to evaporate again. And there is no inherent cause in the physical and chemical properties of water which will bring this cycle to an end. As long as the present physical conditions exist, the cycle must continue. So it is, he claims, with the life cycle of a Protozoön; *i.e.*, division, growth by assimilation, division again — and so on without end; there being no inherent cause in the constitution of the protoplasm which will cause it to fall short of its cycle and physiologically decline in fertility and vigor. He does not mean that such unicellular forms cannot be starved to death, crushed out of existence, devoured, or killed by disease. These are rather accidental than natural deaths. Neither would he claim that in no case have individuals by insufficient nutrition or unfavorable surroundings acquired the seeds of death, so that their race has been limited, and finally become extinct. This again is accidental rather than natural death. He claims only that since life has existed in these forms, it has passed unbroken from one generation to another down to today. The material of which the individual is made may change, but in all cases it is animated by the same life.

Now no one doubts that the Metazoa have at some time in the remote past been evolved from such potentially immortal Protozoa. But the life of all of the Metazoa may be divided into *three* periods, — youth, adult life, and old age, during which latter there is clearly a physiological decline in vigor, which is terminated normally by death.

Old age and death then would appear to be something which have been acquired with the development of the Metazoa from the Protozoa. Exactly for what purpose, and how, death has been instituted, are questions which Weismann endeavors to answer.

But first let us compare the life history of a Metazoön with that of a Protozoön, and see whether there is anything in the Metazoa which is comparable to Protozoön immortality. All Metazoa start their individual lives from an ovum, which is a single cell, and may well be compared to a Protozoön. After fertilization this cell or ovum divides into two, then into four, then into eight cells, and so on, thus giving rise to a very large number of cells, which, as development progresses, differentiate to form the tissues and organs of the embryo. Now of these cells in the embryo Weismann distinguishes two different kinds; viz., the germ cells, which lie in the generative glands of the animal, and the somatic cells, which form all other organs and the body itself. During early youth the germ cells remain dormant. When, however, adult life is reached, they develop, and under proper conditions, such as fertilization, etc., each one is capable of producing a new organism, with germ cells and body, while the body itself grows old and dies. The germ cells of the second generation do not die, but produce a third, and the third a fourth generation, and so on. The body of each of these generations must grow old and die, but the germ cells themselves, if allowed the proper physical surroundings, do not. They go on germinating, and so produce generation after generation.

Hence it would appear that the germ cells of the higher organisms are exactly parallel to the entire body of the lower forms, and like them are endowed with potential immortality, and that this immortality has never been broken by death since life first existed on this earth. In other words a Metazoön equals a colony of Protozoa plus a perishable body.

Death, then, is something secondary; an adaptation which has been acquired during the evolution of the mortal, from the lowly immortal forms.

Weismann suggests that this adaptation of death has been brought about in the following manner, by what he terms *panmixia*, or the cessation of natural selection. It is generally admitted that congenital variation in the size and form of every organ or part occurs constantly, and further that such variation is liable to be inherited; also that species are formed and maintained by natural selection preserving the most favorable variations. For instance, the large and powerful wings of wild ducks and geese are an absolute necessity to them, not only to escape enemies but to migrate from the sub-tropics to the arctic zone as the change of climate and scarcity of food necessitate. With weak wings, a wild duck or goose would have but poor chance of competing with its companions in the stern struggle for existence which is a part of its life. In the stormy northern hemisphere in which most of these birds breed, strong powers of flight are of vast advantage, and when the time for migration comes, an absolute necessity.

Now if some of the young of these birds vary even a little from the parental type, and have even stronger wings than the average, so much the more fortunate they; if, on the other hand, the wings are below the average, there is just so much less chance of their accomplishing the migration safely and returning another year to breed and transmit to their offspring their short-winged peculiarity. In these birds it would seem that natural selection compels strong powers of flight, and that the strong wings which characterize them are maintained by a survival of the fittest. This is a well-known law.

Now without doubt the domestic ducks and geese are descended from wild forms; yet they fly but poorly. Indeed, actual comparison shows that the wing bones are smaller in the domestic than in wild ducks. But in domestic ducks the power of flight is but little used. It is of no advantage to it to fly far and fast, and hence in these forms, the young with small wings would be as apt to survive and transmit their individual variation as would the long-winged ones, and as a consequence of this free interbreeding of the long and the short winged, the general average of length of wing would deteriorate. In other words, natural selection would cease to compel long wings, and *panmixia*, or the "mingling of all," would allow them to degenerate. There are several kinds of wild bird — the dodo, penguin, apteryx, ostrich, — incapable of flight, and all doubtless de-

scended from birds with functional wings. This may be accounted for if we can suppose that gradually the ancestors of these forms, by change of habit, climate, or surrounding conditions, were able to obtain their food with but slight use of the wings. Then the survival of only the strong of wing would cease, and *panmixia* would cause the wings to degenerate.

Without doubt the eyesight of animals which live in the light of day is maintained in its perfection by the action of natural selection. Take, for example, the birds of prey, such as the eagle or vulture, which have exceedingly keen sight, by means of which they detect their food at great distances. A near-sighted vulture would have but poor chance of competing successfully with its more fortunate companions. Another example may be found among savage races of men. It is often asserted that the North American Indians have eyesight keener than that of most white men. To the Indian this sense is of as much importance as to the vulture or eagle, for like them, he is dependent on the chase for his livelihood. A myopic or weak-sighted Indian could rarely survive to transmit his weakness to the race. In both these cases the keen sight has been produced by a survival of only the fittest; and if this natural selection were removed, the result would be a reduction in the power of the organ in question. We are, of course, descended from keen-sighted savages, but civilization has introduced other means of support, in which the myopic individual stands on equal terms with the man of normally, or even abnormally, good sight. Here again *panmixia* has caused an organ to degenerate.

Similar cases are to be found among animals which live in subterranean caves where total darkness prevails. Some of these caves have existed since the Pliocene period, and it is probable that their present inhabitants, insects, crustacea, amphibia, and fish, have lived and bred there for thousands of generations. Many of these are blind, having but rudimentary eyes. Some are also colorless. Now to animals living in the light of day, color is either protective, or of some advantage; therefore it is maintained by natural selection. But if congenital variation should produce animals with less vivid hue, these would, of course, survive in total darkness as well as their more brilliant brethren; and by breeding with them, the colors of all would gradually be reduced.

And so with their eyesight. What advantage is eyesight in total darkness? It is interesting to note how in these caves natural selection has been enlarging and developing organs which are of use in darkness, even while in the same species *panmixia* has been reducing those which are useless. In the Mammoth Cave there is a blind, colorless, and wingless grasshopper. Eyesight, color, and wings have been aborted by *panmixia;* but it has singularly long and delicate antennæ, which serve as feelers, and which are not nearly so conspicuous in forms living in the light of day. It would seem then that natural selection maintains only what is of advantage to an animal, and when it neglects an organ, that organ tends to degenerate by the action of *panmixia;* and these laws apply to the physiological functions of organs, as well as to the structure of the organs themselves.

Is immortality essential to the Protozoön? Weismann claims that it is, for the whole body is the germ-cell by which the race is perpetuated; hence, if the body should lose its immortality, the race would become extinct. Immortality then is retained by natural selection.

Now among the Metazoa the case is quite different. In these forms immortality is only essential to the germ cells. The body is merely the bearer and protector of these precious germs, and immortality is in no sense essential to it; hence it has not been maintained by natural selection, but has disappeared through *panmixia.*

Weismann suggests that the Metazoa, with their immortal germ and perishable body, have been evolved from the wholly immortal unicellular form something as follows: Many Protozoa form cluster-like colonies. In such groups those on the outer side must obtain food more readily than those in the center; hence the colony would become gradually differentiated into feeding cells on the outer side and reproductive cells on the inner side, the feeding cells supplying the reproductive cells with nutriment just as the digestive cells of Hydractinia supply the rest of the colony. The cells that are thus supplied with food would have no use for mouth, cilia, etc.; hence they would lose them, but might retain their reproductive powers. If these central cells retained their immortality, there would be no necessity for the feeding cells doing so also; and if natural selection does not compel the retention of a physiological character, it degenerates, just as a useless organ degenerates.

Certain of the lower forms, such as Volvox, suggest this manner of evolution of the Metazoa from the Protozoa. Volvox is a hollow sphere of cells, each of which is provided with a couple of long flagella, by means of which the colony swims. Some of these cells pass to the center of the sphere, and there undergo certain changes in form, becoming, in fact, the reproductive cells of the colony. When they are ripe, the rest of the colony withers up and dies. Hence we find in Volvox the first approach to a differentiation into germ and somatic cells.

Since Weismann made this startling assertion, that death is not an attribute of all living organisms, much opposing evidence has been brought forward. Most prominent and recent among his opponents is E. Maupas, of Algiers, who, after extensive study of some of the Infusoria, asserts that degeneration and death occur as normally among the Protozoa as among the Metazoa. Before entering on his experiments, Maupas first determined very carefully the habits of the different species which he chose for study. He found out the temperature to which they were best adapted, and the kind of food on which they throve best. Then he took a single individual and isolated it on a glass slide, on which it could be studied. This slide was kept over a dish of water, in a warm, damp chamber in which the air was so thoroughly saturated with moisture that evaporation was reduced to a minimum. During its confinement the animal was fed on its favorite food, and in every particular what seemed to be the most suitable conditions were maintained. He found that at the end of seven days there were no less than 935 Infusoria in his culture. One of these 935 he isolated and kept as he had the first. In four days this single one had increased to 230. One of these was isolated in the same way, and this process of isolating and confining one individual of each brood was continued through a large number of generations. He shows the rapidity of increase to be something almost incredible. Calculations show that in six and a half days a single Stylonichia might produce by fission a mass of protoplasm which should weigh one kilogram, and that in thirty days the number of kilograms would be represented by 1 with forty-four zeros, or a mass of protoplasm a million times larger than the volume of the sun!

Cultures were made of no less than twenty different species of Infusoria, and were maintained during periods of time varying in

different cases from two weeks to between four and five months. He found that after from fifty to one hundred generations had been produced by fission, there was clear evidence of a physiological decline, which seemed to indicate the approaching extinction of the culture. He withdrew some of the Infusoria from the culture and allowed them to mix with others of a different origin. With these they conjugated, and their full vigor seemed restored. If, on the other hand, they conjugated among themselves, observation showed that decline was so far advanced that the culture was doomed.

Soon the animals produced by fission were smaller — often being less than half the normal size. At the same time what might be called pathological changes began to appear. The cilia were absent on parts of the body, and the Infusoria seemed weaker and less able to digest food. In some species the micro-nucleus underwent changes, finally falling to pieces — a phenomenon which not unfrequently occurs in the cells of the Metazoa when the tissue is undergoing degeneration. Also the macro-nucleus was found to undergo marked pathological changes, finally breaking down and disappearing.

When this degeneration, which Maupas calls senile degeneration, reaches its maximum, nutrition becomes impossible, and death follows. Thus it would appear that the life of these animals is cyclic. During the period of reproduction, which is the adult life of the animal, a sort of physiological decline takes place, and this decline can be repaired only by conjugation.

Now if, during these experiments, the animals have not been injured or poisoned, it would seem at the first glance that Maupas had proved that among the Infusoria death may occur normally; and on the strength of his experiments he holds up Weismann's theory to ridicule. This is evidently unfair, for although he has shown that Weismann was wrong in ascribing immortality to the Infusoria, yet there are many well-known unicellular forms in which neither a physiological decline nor a process of rejuvenation has been observed. The Bacteria, the Cyanophyceæ, and Yeast, increase by budding, spore-building, and fission; conjugation does not occur, and unless their life-history is much less well known than bacteriologists and botanists think, these forms are potentially immortal. The Infusoria, on the other hand, are the highest and most differentiated of unicellular forms. They have organs of locomotion, mouth, pharynx, some sort of excretory apparatus, myophanes (muscle-like structures), trichocysts,

etc.; while in the lowest organisms none of these organs are to be found. Further, the Infusoria have a macro-nucleus which Maupas shows is vegetative in its function, and a micro-nucleus which is generative. If the macro-nucleus is lost, nutrition fails; and if the micro-nucleus is lost, conjugation is impossible. In many of the lower Protozoa no such differentiation has been observed, and there is merely one nucleus, which is surrounded by a mass of protoplasm.

In a recent article Bütschli maintains that in the Bacteria the whole body is the nucleus, and that the surrounding mass of protoplasm, such as characterizes the Rhizopods, is absent. Between the Bacteria and Infusoria there is a wide gap in the zoölogical scale. Is it not possible that as the Infusoria were evolved from lower and simpler forms, the process of conjugation was first acquired? that when, in the cycle of metabolic changes, the protoplasm fell short of the point from which it started and to which it should return, this deficiency was made up by foreign substance obtained from an individual of different origin, and therefore of different material? Those of the primitive forms which retained their original immortality have left lineal descendants which we know today as Bacteria. Those which in a measure lost that power have either become extinct, or else acquired a habit of rejuvenescence by conjugation. In other words, those to which it was an advantage to retain their immortality have retained it, and those which varied in such a manner that immortality could be advantageously replaced by rejuvenescence have, by the action of natural selection, undergone this modification. If this is so, Weismann's error is not in claiming that death was an adaptation, but in asserting that all unicellular forms are immortal.

Weismann, however, is not ready to admit this. He claims that conjugation is a necessary condition of the animal's life, just as fertilization is a necessary condition for the survival of an ovum, and if conjugation is denied, the death in consequence is accidental and not natural; further, that the fact that conjugation is necessary does not imply that the protoplasm is not potentially immortal. He seems, however, to overlook the fact that a certain physiological decline has taken place, and that if there is any physiological decline the cycle of life is incomplete — therefore the seeds of death must exist inherent within the life of the animal. It is, then, for the present, impossible to speak of the Infusoria as potentially immor-

tal, and to claim that that portion of the Metazoa which undergoes physiological decline has no equivalent in these forms.

Still another and earlier opponent of Weismann has urged serious objections to this theory of the origin of death. Professor Charles S. Minot was the first to maintain — and many have taken up his suggestion — that Weismann is fundamentally wrong in comparing the life history of a Metazoön, which is a complex colony of cells, with that of a Protozoön, which is a single cell. Minot urges that an individual Metazoön is comparable to a colony of Protozoa, not to a single cell. If this be so, then the death of a Metazoön (a colony of cells) has its only homologue in the degeneration and death of a culture of Protozoa. The Metazoön colony is the product of a single germ cell, as is also the whole culture of Infusoria.

This comparison seems safe between the Metazoa and those forms of Protozoa which conjugate, and in which senile degeneration occurs. But how is it when we bring those which do not conjugate under consideration? If this view be correct, then a single Metazoön is equivalent to all of a species of Bacterium which may arise through generations of fission. As far as our knowledge goes, these Bacteria are immortal, and their numbers almost infinite. We know, on the other hand, that nothing but the germ cell of the Metazoa possesses this immortality and vast power of reproduction.

But to return to Weismann's views. If death is not something which is inherent in living matter, but which is acquired, how is it that the length of life differs so markedly in different species? Weismann answers that the age which an animal may attain has been determined by natural selection, and also that the power of reproduction and length of life are correlated. In order to understand this view it is necessary to compare the length of life with the reproductive powers of different animals. Birds, as a rule, live to a surprisingly great age. Even the smallest singing birds live for ten years; while some live for twelve, or even eighteen years. A partridge lives from twenty to twenty-five years. A pair of eider ducks was observed nesting in the same place for twenty years, and it is believed that these birds often reach the age of a hundred. The same cuckoo was recognized by its peculiar note in the same forest for thirty-two consecutive years. Birds of prey becon.e much older, for they outlive more than one generation of men. A white-headed vulture was kept in a zoölogical garden in Germany for one hundred

and eighteen years; and many examples of eagles and falcons reaching an age of over a hundred years have been recorded. Humboldt mentions a parrot from the Orinoco, of which the Indians told that none could understand it, for it spoke the language of an extinct race.

Now let us compare the length of life and reproductive powers of the partridge and an eagle, and see if there is any reason why one should live longer than the other. The partridge lives a little more than twenty years, and each year lays about twenty eggs. Hence a pair of partridges may produce about four hundred eggs in their lifetime. This is at the rate of two thousand in a hundred years. Yet, since the number of partridges in the forest does not increase, three hundred and ninety-eight of these eggs, or young, must be destroyed in twenty years; while but two survive to take the place of their parents. The eggs and young are destroyed by beasts and birds of prey. If these enemies increased very much in number, the partridge would become extinct, unless it laid more eggs.

It would appear, then, that the partridge lays just eggs enough to ensure the continuance of its race; and, this being accomplished, death removes it. Many species have doubtless become extinct through the insufficiency of their reproductive powers. The number of offspring which, under ordinary conditions, would have ensured perpetuation, has proved insufficient when their enemies increased or the environment became unfavorable. The supply must be equal to the demand.

Now for the eagle. The eagle is one of the most powerful of birds, and builds its nest on such inaccessible cliffs that eggs and young are comparatively safe from marauding animals. Many, however, are destroyed by late frosts and snows. To be on the safe side, let us fix the duration of life of the eagle as sixty years; and of this, ten years are spent in immaturity. Hence there are fifty years of its life during which it reproduces. If the eagle lays but two eggs a year, a pair of eagles would produce one hundred during their lifetime. In a hundred years two hundred eggs against the partridge's two thousand; therefore the partridge produces ten times as many young as the eagle, and it is safe to say that the partridge has ten times as many enemies. If the life of either were short-

ened, the race would die out unless the power of reproduction were increased, or the struggle for existence became less severe.

Many sea-birds, such as the petrel, auk, and gannet, lay but one, or at the most two eggs a year. Any one who visits a locality where these birds breed must be struck with the enormous number of eggs or young which are destroyed. The eggs are often laid on the bare rock on projecting ledges of a cliff, so that the slightest movement will precipitate them to the beach below. Every disturbance among the breeding birds is marked by a small avalanche of eggs or young, so that the beach below is strewed with broken eggs and mangled remains. If these birds were not long-lived, they would soon become extinct. Now all of these birds live much longer than mammals of a far larger size. The lion lives thirty-five years, the sheep fifteen, the fox fourteen, the squirrel or mouse about six. Most of these animals are much more fertile than the birds, and the young are much less exposed to dangers. The bird's egg is exposed from the time it is laid, while the young mammal is protected during its development.

Only the very largest of the mammals, such as the whale, the elephant, and possibly the rhinoceros, live as long as these birds. The elephant may live for a hundred or perhaps a hundred and fifty years, and reaches maturity when about thirty. A pair produce but a single calf about every ten years; hence, during their lifetime, a pair of elephants contribute but ten or a dozen young to the race.

Wallace shows that we are living now in a zoölogically impoverished world. Almost all of the largest and strongest forms have recently become extinct: in Europe, the great Irish elk, the sabre-toothed tiger, cave-lion, rhinoceros, hippopotamus, and elephant; in North America, equally large felines, horses, and tapirs larger than any now living, a llama as large as a camel, mastodons and elephants, besides a large number of huge megatherians; in South America, an even greater number of megatherians, huge armadillos, a mastodon, large horses and tapirs, large porcupines, two kinds of antelopes, numerous bears and felines, besides the sabre-toothed tiger.

Remains of all these are found in the recent deposits, and these animals lived till shortly before the northern continents were encased with the ice of the glacial epoch. It is possible that a change of climate, due to the growing cold from the encroaching ice-belt,

affected the flora. This would, of course, affect the food supply, and so tend to lessen the reproductive powers, and shorten the lives of the individuals. Lessened reproductive power and shortened lives of the individuals would surely result in the extinction of the race, and in this way the destruction of these forms may be accounted for. The starvation of the individual is therefore synonymous with the starvation of the race.

The above considerations seem to warrant the theory advanced by Weismann: *first*, that unicellular organisms may be potentially immortal; *second*, that death is an adaptation; and *third*, that the length of life and the reproductive powers of animals are correlated.

THE APPLICATION OF THE ALPHA-SULPHONIC ACID OF NAPHTHALENE[1] *TO THE BATING AND PURING OF HIDES AND SKINS.*

By PETER S. BURNS, S.B., and CHARLES S. HULL.

Improvements in the arts during the last half century, and more especially as regards the manufacture of leather, are confined almost entirely to methods for decreasing the cost through the adaptation of machinery; and little progress has been effected in the chemical methods employed, either to apply the present agents with greater economy, or new agents that may introduce economy of material or betterment in the quality of the finished product.

To briefly summarize the process of tanning, it may be said that the softened hide or skin is immersed in a solution of slaked lime, or other alkali, for a period varying from three days to a week. It is then taken out, and the hair, which is loosened by the action of the alkali, is mechanically removed. The skin would now be ready for the tanning liquors, were it not for the presence of the lime, which the fibre and gelatine of the skin absorb, and which it is necessary to remove before the tanning of the skin may be attempted. The necessity of removing the lime arises from the fact that, with it, tannic acid forms an insoluble compound, dark in color on account of the coloring matter with which it is precipitated, and also because of the oxidation of the tannate of lime in the presence of the alkali of the hide. It is found that besides becoming dark in color when lime is present, the hide tans very slowly, and that when the process of tanning is complete, the leather so produced is brittle and tender. It is in the removal of the lime that one of the greatest losses of material in the present methods occur, through a loss of

[1] This alpha-sulphonic acid of naphthalene is sold for tanners' use under the name "Acrilene Antiseptic Bating and Puring Acid."

gelatine to the hide structure itself. The processes now in use for the removal of the lime are invariably accompanied by and dependent upon the action of bacteria, which subsist upon and consume the hide while it is subjected to their action. These methods consist in immersing the hide in an animal or vegetable putrescent or fermenting solution, and the agents usually employed are hen, pigeon, or dog manure, sugar, glucose, and bran. The action of these agents by the dissolution of the gelatine results in the depletion of the hide, and allows of the removal of the lime. This is effected principally by mechanical means, such as working and washing, and is rendered possible only through the action of the bacteria on the hide structure itself, which becomes wasted and relaxed. Our experiments show that the removal of the lime is assisted by the carbonic and phosphoric acids and their ammonium salts, which were found to be present in the putrescent solution, as well as by formic, acetic, propionic, lactic, butyric, or other similar acids incident to the fermentations which are commonly resorted to. The loss of gelatine incident to these methods was found by experiment to amount to, as a minimum, from two to three per cent. of the weight of the dry hide submitted to their action. This loss is nearly doubled in the tanned leather, owing to the fact that leather may be considered to contain one half its dry weight of tannic acid. Our experiments on this subject have been made with the object of procuring an antiseptic chemical agent that would remove the lime without the attendant action of bacteria, and consequently without loss of gelatine to the hide. It is evident that the best way of removing the alkali is by converting it into a soluble salt by the use of an acid; but the common mineral acids whose lime salts are soluble act upon the hide in a most unexpected way, causing it to swell and assume a transparent and gelatinous appearance, thus rendering the hide useless for making most kinds of leather. The acids produced by the fermentation of bran, sugar, and glucose, give similarly unfavorable results; from which fact we are led to the conclusion that, when hides are submitted to *fermenting* solutions of these same agents, the depletion is due to the action of the bacteria, and not to the direct action of the acetic and other acids simultaneously generated. The organic acids were widely investigated, and from among the long list alpha-naphthalene-sulphonic acid alone was found to combine with and remove the alkali of the hide without mechan-

ical aid, leaving the hide soft and white, and without entailing any loss of gelatine. Why the sulphonic acid of naphthalene should remove the lime without causing the swelling and gelatinous appearance produced by the other acids, of which, perhaps, citric acid produces the most marked effect, we are unable to say at present. The antiseptic properties of naphthalene-sulphonic acid, which are about the same as *a*-phenol-sulphonic acid, make it most desirable for the use to which it is proposed to put it, while the low cost of the materials from which it is manufactured should bring it within the tanner's reach. The application of naphthalene-sulphonic acid to the manufacture of leather on a larger scale than was possible at the laboratory was made at the works of one of the largest calf-skin tanners in New York State. Two lots of calf-skins were taken for the trial, alike in condition and character. Each lot numbered 150 skins, and each weighed 880 pounds in the hair. Each skin of each lot was then given a distinguishing mark, and after the treatment with lime and removal of the hair, one lot of 150 skins was subjected to the hen-manure drench and the other lot to a three per cent. solution of the sulphonic acid of naphthalene. Both lots were then mixed and tanned in the same vats for the purpose of securing equal conditions. When the tanning was completed the skins were sorted into their original lots and weighed. It was found that the skins subjected to the hen-manure solution made 255 pounds of dry leather, which, when stuffed, weighed 395 pounds; and that the skins treated with the naphthalene-sulphonic acid made 266½ pounds of dry leather and 413 pounds of stuffed leather; both lots having been stuffed in the same operation — a net gain in finished weight of $4\frac{55}{100}$ per cent. It was also noted that the hen manure skins were very apparently lighter in the flanks and shoulders than those bated with the sulphonic acid. The safety and certainty with which this operation may be repeated by the most ignorant of workmen, thereby insuring equality in the value of the finished product, give it a great advantage over the present agents, which, aside from being very nasty and offensive, even to workmen familiar with their stench, are continually accompanied, especially in summer, by danger to the stock, even in the hands of the most experienced workmen.

LABORATORY OF THE INSTITUTE OF TECHNOLOGY,
Boston, May, 1891.

A DELICATE TEST FOR ALUM IN POTABLE WATER.

By ELLEN H. RICHARDS.

In 1878–79, while making examinations of some food materials for the State Board of Health of Massachusetts, it was necessary to test for alum in bread, and also in baking powders, in the presence of sodium carbonate. The method which was found to be uniformly successful is given on page 161 of "Food Materials and their Adulterations" (Estes & Lauriat, Boston, 1886).

When alum came into use in various methods of purifying water on a large scale, it became important to find a test for it of as great delicacy as that for ammonia; for by the usual methods chemists had often reported no alum in these filtered waters.

Mr. George L. Heath, Assistant in Sanitary Chemistry in 1888–1890, successfully modified the logwood test for alum referred to above, so that it can be applied to potable waters.

On the addition of alum to natural waters, there is a precipitation of alumina in proportion to the amount of carbonates or bicarbonates present in the water. It is, of course, only the alum in excess of the amount decomposed that is to be tested for. The precipitation of the alumina is a gradual process, and a water that will give the test for alum immediately after filtering may give none after twenty-four hours, since the alum may have been all decomposed in the mean time. It is not unfrequently noted that the effluents from filters using alum which are originally clear, become cloudy on standing, in consequence of the separation of aluminum hydrate.

On the addition of alum to brown surface waters there is also a precipitation of alumina by the coloring matters, tannin, or other substances, and in this case also only the excess of alum is to be tested for. For instance, a sample of the Cochituate supply, of moderately deep color, to which twenty-five milligrams of alum to the

liter had been added, when filtered gave no reaction for alum, even when 2.5 liters were concentrated for the test. An addition of thirty milligrams to the liter could be detected without difficulty.

The method is as follows: To 25 cc. of the water to be tested (concentrated from one liter or more, if necessary) is added a few drops of freshly prepared logwood decoction; any alkali is neutralized and the color is brightened by the addition of two or three drops of acetic acid. By comparison with standard solutions, the amount of alum present may be determined. One part of alum in 1,000,000 of water can be detected with certainty. In cases of greater dilution, concentration of several liters may be necessary to obtain a decisive test. The logwood chips yield the right color only after having been treated with boiling water two or three times, and rejecting the successive decoctions. The first portion gives a yellow color, the third or fourth usually a deep red.

NOTES ON THE ACTION OF HYDROBROMIC AND HYDRIODIC ACIDS ON TIGLIC AND ANGELIC ACIDS.[1]

By HENRY P. TALBOT, Ph.D.

In his article on the action of hydrobromic acid and of bromine upon tiglic and angelic acids, published in 1879, Pagenstecher[2] states that the additionproducts thus obtained from the angelic acid were identical with those yielded by the tiglic acid under the same conditions. This he proves in the case of the hydrobromic acid by showing the identity of the pseudobutylenes produced by treating these additionproducts with sodium carbonate.

In direct contradiction of this statement, Pückert[3] asserts that he obtained a dibromangelic acid which differed in its physical properties and in its decomposition products from the dibromtiglic acid prepared under the same conditions. Pückert characterizes the two new acids as geometric isomers, and shows the relation of both the dibrom-additionproducts, and of the brompseudobutylenes derived from them, to the geometrically isomeric acids which formed the starting point.[4]

The results of Pückert's investigation led Wislicenus[5] to express the opinion that the hydrobrom-additionproducts, which Pagenstecher regarded as identical, would prove to be geometric isomers, or that such isomers could be isolated.

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[1] A part of an Inaugural Dissertation entitled "Ueber das Verhalten der Tiglinsäure und der Angelicasäure gegen Bromwasserstoff und Jodwasserstoff," presented to the Faculty of the Leipzig University, Leipzig, 1890.

[2] Liebig's Annalen, 195, 108. [3] Liebig's Annalen, 250, 243.

[4] In a recent number of Liebig's Annalen der Chemie (259, 1) Fittig re-affirms the accuracy of Pagenstecher's work. Accordingly the question of the existence of a dibromangelic acid must be considered as still open.

[5] Liebig's Annalen, 250, 254.

Some years before Pückert's publication, Schmidt[1] had studied the action of hydriodic acid upon both tiglic and angelic acid, and states that he obtained a product from the angelic acid which differed essentially in its melting point from that prepared from the tiglic acid under similar conditions. Schmidt did not, however, succeed in preparing a pure product which he could characterize as iodohydroangelic acid, although he rendered the existence of such a compound very probable. He states that both the iodohydrotiglic acid and the product from the angelic acid yield the same methyl-ethyl-acetic acid upon reduction. Fittig[2] asserts that both additionproducts give the same pseudobutylene on treatment with sodium carbonate. He prepared his product in the same way as Schmidt, and does not state that he was successful in purifying the iodohydroangelic acid.

The recent discovery and identification by Mellikoff and Petrenko-Kritschenko[3] of four isomeric chloroxyvalerianic acids resulting from the action of hypochlorous acid upon the two organic acids in question, and exactly corresponding to the requirements of the theory of Le Bel and van't Hoff, made it appear still more probable that a review of Pagenstecher's and Schmidt's work would lead to the isolation of isomeric bromhydro- and iodohydro-additionproducts, and throw additional light upon the constitution of the tiglic and angelic acids themselves.

Such was the object of the present investigation, which was carried on at Leipzig, under the direction and at the request of Professor Wislicenus, to whom the writer is under great obligation for kind assistance and suggestion.

The tiglic and angelic acids, which were required as a starting point, were prepared as follows:

The angelic acid was obtained from the oil of the camomile (*Anthemis nobilis*), known in German trade as *Römisch Kamillenöl.* The oil was saponified in portions of 500 grms. each, by mixing with a solution 250 grms. of caustic potash in 125 cc. of water. The saponification was accomplished chiefly by vigorous shaking for a half-hour without the application of heat, beyond 5 – 10 minutes upon the steam bath at the close, to complete the reaction. A longer heating lessens the yield of angelic acid. After the removal of the alcohols and acidification with hydrochloric acid, the greater portion

[1] Liebig's Annalen, **208**, 253. Berichte der deutschen chemischen Gesellschaft, **12**, 255.

[2] Liebig's Annalen, **216**, 261. [3] Liebig's Annalen, **257**, 116.

of the organic acids separated as an oily layer. These were removed by a separating funnel, and the remainder extracted with ether. The acids were then subjected to fractional distillation, and after each fractionation the distillates were exposed to a temperature of 0°– 5° C. The angelic acid separated from the portions distilling between 181°–184°, 184°–187°, 187°–190°. From the portions distilling above 190°, tiglic acid separated. The fractionation and cooling were repeated until the yield of angelic acid was so small as to make it useless to continue. The angelic acid was thoroughly pressed and twice recrystallized from distilled water.

The yield was 11% of the weight of oil used. The thoroughly dry acid melted sharply at 45° C.

The tiglic acid required was partially prepared by the reduction of methyl-acetacetic ether (Rohrbeck,[1] Pückert[2]), but the greater portion was obtained from the *Römisch Kamillenöl*, after the repeated fractionations. The fractions above 190° gradually increase in volume, and from these tiglic acid crystallizes readily. Moreover, the mixture of acids from which the angelic acid will no longer crystallize was heated for twenty hours or more to the boiling point, and from this mixture (in which the angelic acid gradually changes to tiglic acid), considerable quantities of the latter acid were obtainable. The tiglic acid was recrystallized from distilled water, and after thorough drying melted sharply at 65° C.

Both the tiglic and the angelic acids were thoroughly dried before they were subjected to the action of either hydrobromic or hydriodic acid.

A long series of carefully conducted experiments, carried on under varying conditions, upon the action of hydrobromic acid upon angelic acid, yielded but one result, viz.: the additionproduct, after purification, could not be distinguished in any of its physical properties from that obtained from tiglic acid under the same conditions, although its purification was always attended by greater difficulty owing to the presence of a minute quantity of some other substance.

This, in itself, confirms the results obtained by Pagenstecher. At the same time, the fact which will be spoken of later, that different products do result from the action of hydriodic acid upon these same organic acids (as well as the work of Mellikoff and Petrenko-Kritschenko already referred to), seems to admit of the hope that a bromhydroangelic acid may yet be isolated.

[1] Liebig's Annalen, 188, 235. [2] *Ibid.*, 250, 243.

The experiments with hydrobromic acid may be briefly outlined as follows:

Hydrobromic acid was allowed to act upon the tiglic and angelic acids in aqueous solution, and also in solution in ether, chloroform, and low-boiling petroleum products (petroleum ether). The addition of the hydrobromic acid took place very slowly, except in ethereal solution. In the latter case the addition was nearly complete at the end of an hour, whereas it required many days in the other solvents. Experiments also indicated that even in this short time the contact with hydrobromic acid had caused the angelic acid to change into a derivative of tiglic acid.

The action of the hydrobromic acid in the ethereal solution yielded unexpected results, in so far as the product, in all cases in which the acids remained in contact with the ether for more than an hour, was an ethyl ester of bromhydrotiglic acid instead of the free acid. The formation of the ester was the result of a secondary reaction between the hydrobromic acid and the ether, in which ethyl alcohol and ethyl bromide were formed. The alcohol, in the presence of the hydrobromic acid, formed an ester with the bromhydrotiglic acid. Tiglic acid was the only acid which could be obtained from these esters by saponification.

The additionproduct obtained from the angelic acid when treated in aqueous solution, or in petroleum ether, agreed in melting point, solubility in petroleum ether, behavior when melted under water and when rubbed together with tiglic or angelic acids, with the bromhydrotiglic acid.

The same was true of the products obtained when chloroform was the solvent, with one exception. In this medium the addition of the hydrogen and bromine took place with great slowness. In one instance, after standing with hydrobromic acid for forty-eight days, a small quantity of an oily substance was obtained from angelic acid which was soluble in sodium carbonate solution, and contained 44.04% bromine (theoretical for $C_5H_9O_2Br$., 44.19%). This body may correspond in character with that observed by Fittig,[1] which formed with the dibromtiglic acid on the addition of bromine to angelic acid, and which he regards as a possible dibromangelic acid. In this series of experiments the solutions were allowed to stand in a cool place and in diffused light; never in *direct sunlight.*

[1] Liebig's Annalen, 259, 26.

The action of hydriodic acid upon the tiglic and angelic acids was first tried in aqueous solution according to the directions given by Schmidt.[1] A quantity of aqueous hydriodic acid was completely saturated with the gas at 0°. Of this concentrated acid 80 grms. were taken for each 10 grms. of tiglic or angelic acid. The solution of these acids followed quickly, and after standing eighteen hours away from the light, the solution containing the tiglic acid had become a semi-solid mass, while a layer of oil, which crystallized on shaking, had separated from the solution of the angelic acid. The crystals were collected on a filter of glass-wool, washed, pressed and dried over soda-lime.

The iodohydrotiglic acid, after a single recrystallization from petroleum ether, melted sharply at 86.5°–87°, a half-degree higher than the melting point recorded by Schmidt.

Analysis:

0.3675 grms. substance yielded 0.3780 grms. Ag I

Calculated for $C_5H_9O_2I$	Found.
55.70% I	55.56% I

The yield amounted to 87% of the theoretical quantity. The crystals from the angelic acid after one recrystallization melted at 44°–50°. After repeated recrystallization they melted from 46° to 70°, chiefly between 46° and 50°. Schmidt records a similar behavior of his product from angelic acid.

It was only possible to obtain a small quantity of crystals melting between 84° and 86.5°, which were undoubtedly iodohydrotiglic acid. It was evident from its behavior that this product was not a single substance, but a mixture. To determine whether the mixture was one of unchanged acid with the iodohydro-acid, a determination of the iodine was made which resulted as follows:

I. 0.2080 grms. substance gave 0.2149 grms. Ag I
II. 0.1948 " " " 0.2024 " Ag I

Calculated for $C_5H_9O_2I$	Found.
55.70% I	I. 55.81
	II. 56.10

From these percentages it appears that the mixture is composed of two or more substances corresponding to the symbol of iodohydro-angelic acid. This is a direct confirmation of Schmidt's results.

Wislicenus has established the fact that the presence of moisture

[1] Liebig's Annalen, 208, 254.

frequently leads to the formation of abnormal products in reactions intended to produce geometric isomers, especially in the case of the less stable of these isomeric compounds. He has, moreover, proved by a series of experiments that the presence of direct sunlight also leads to the formation of abnormal products. Fittig[1] publishes some striking results bearing on this point.

In the second series of experiments, an attempt was made to exclude both light and moisture. The exclusion of the moisture was attained by the use of chloroform as a solvent. Chloroform, free from alcohol, was saturated with dry hydriodic acid gas, and the amount of the hydriodic acid in each cubic centimeter determined by titration. The quantity of the organic acids was so chosen that a considerable excess of the hydriodic acid was present. The solution stood, away from the light, and at 15° C., for seven days. The chloroform and the excess of hydriodic acid were then removed by evaporation in a current of dry air, the solution being protected from the light during the evaporation.

The crystals remaining from the solution of the tiglic acid proved to be identical with those prepared from hydriodic acid in aqueous solution. The yield was 95% of the theoretical quantity.

The yield of crystals from the angelic acid amounted to 98% of the theoretical quantity. Their purification presented greater difficulty than that of the iodohydrotiglic acid, owing to their ready solubility in all organic solvents.

By solution in slightly warm petroleum ether and repeated partial evaporation of the solvent in dry air, four crystallizations were obtained. The crystals of the first portion melted from 58.5°–60.5°; of the second, at 56°–58°; of the third, at 51°–56°; and of the residue after the complete removal of the solvent at 42°–50°. The latter portion was in all respects similar to the mixture obtained from the hydriodic acid in aqueous solution, and resisted all attempts to separate it into its constituents. An analysis showed that it contained 55.94% I (theoretical for $C_5H_9O_2I$, 55.70%).

From the crystals melting between 51° and 60.5° a considerable quantity of a substance melting sharply between 59.5° and 60.5° was obtainable by recrystallization. This substance, in distinction from the mixture, showed all the properties of a uniform body.

[1] Liebig's Annalen, **259**, 18.

Analyses:

I. 0.1427 grms. substance yielded 0.1473 grms. Ag I
II. 0.2923 " " " 0.3015 " Ag I

Calculated for	Found.
$C_6H_9O_2$ I	I. 55.77% I
55.70% I	II. 55.72% I

These analyses indicated that the substance in hand was an iodohydroangelic acid.

Although the recrystallization was conducted as far as possible out of the light, it was evident that the iodohydroangelic acid gradually changed into iodohydrotiglic acid, as the pure crystals, on warming for a little time with petroleum ether, would lose their sharp melting point without loss of iodine. When both moisture and light were carefully excluded during the whole process of preparation, the yield of pure iodohydroangelic acid was 70% of the total product. The yield was but 54% when light was not excluded. Both the iodohydrotiglic and the iodohydroangelic acids form well-defined crystals.[1]

As an additional means of comparison of these two acids, the solubility of each in petroleum ether and in water was determined.

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[1] Specimens of the crystals of each of these acids were sent to Dr. A. Fock in Berlin, who very kindly examined them crystallographically, and reports as follows:

IODOHYDROTIGLIC ACID.

Crystals belong to the monoclinic system.

$a : b : c = 1.7049 : 1 : 0.8732 \qquad \beta = 82° 21'$

Forms observed:

$a = (100) \infty \breve{P} \infty \qquad c = (001) 0P \qquad s = (101) -\breve{P} \infty \qquad m = (110) \infty P$

$o = (\bar{1}21) + 2\breve{P} 2 \qquad p = (121) - 2\breve{P} 2$

The crystals are tabular in the direction of the orthodiagonal section a. The hemidome s and the hemipyramids p and o occur but rarely. The cleavage, parallel to the orthodiagonal section, is perfect. The position of the optical axes cannot be detected either in the base or in the orthodiagonal section.

IODOHYDROANGELIC ACID.

The crystals belong to the monoclinic system.

$a : b : c = 0.2741 : 1 : 0.2145$

$\beta = 57° 9'$

Forms observed:

$b = (010) \infty \breve{P} \infty \qquad m = (110) \infty P, q = (011) \breve{P} \infty$

The crystals are mostly prismatic in the direction of the vertical axis; a few individuals are tabular in the direction of the plane of symmetry. An optical examination shows that the crystals are almost without exception twins, the twin plane being the orthodiagonal section, so that a purely geometric examination would easily lead one to assert that the crystals belonged to the rhombic system.

The cleavage is perfect in the direction of the clino-diagonal section.

The finely pulverized acid was covered with a quantity of the solvent insufficient for complete solution and allowed to stand for four or five hours at a definite temperature, with occasional shaking. The solution was then decanted, weighed, the petroleum ether removed by evaporation in a current of dry air, and the residual acid weighed.

The results are tabulated below:

IODOHYDROTIGLIC ACID.

Temperature.	Weight of solvent.	Weight of dissolved acid.	Parts acid dissolved per 100 parts solvent.
20.5° C.	18.8250	0.5460	2.90
22°	18.6260	0.5750	3.09
24°	18.0373	0.7310	4.05
IODOHYDROANGELIC ACID.			
20.5°	7.6163	1.2865	16.8
22°	11.8910	2.5095	21.1
24°	12.9013	2.8950	22.4

The difference in solubility is most striking.

The solubility in water was determined by titrating the decanted solution, after weighing, with caustic potash, using litmus as an indicator. The first appearance of a decided blue was taken as the end point. This corresponds approximately to one molecule of the alkali to one of the acid, but the results are probably not accurate within about three or four per cent.

IODOHYDROTIGLIC ACID.

Temperature.	Weight of solvent.	Weight of dissolved acid.	Parts acid dissolved per 100 parts solvent.
20° C.	47.3587	0.3053	0.64
20.5° C.	41.5344	0.2736	0.65
IODOHYDROANGELIC ACID.			
20° C.	43.2717	0.6440	1.49
20.5° C.	38.5124	0.5996	1.55

Here, again, the iodohydroangelic acid shows a decidedly greater solubility.

Finally, the behavior of the two new acids when brought into contact with the original acids was examined, and it was found that when iodohydrotiglic acid was intimately mixed with tiglic acid by rubbing together in an agate mortar, in the proportion of one molecule of each, a mixture results which melts between 44° and 60°. Iodohydroangelic acid and tiglic acid, molecule for molecule, yield a mixture melting between 28° and 32°.

One molecule of iodohydrotiglic acid with one molecule of angelic acid affords a mixture melting between 28° and 62°, while one molecule of iodohydroangelic acid with one molecule angelic acid immediately forms an *oil.*

After the comparison of the properties of these two addition-products, it is not to be doubted that they are distinctly different bodies, differing in melting point, crystal form, and solubility. Experiments have been made upon the decomposition products which these iodohydro-acids yield, which, if confirmed, seem to indicate that the pseudobutylenes produced by the action of sodium carbonate upon these acids are not identical, as Fittig asserts, but can be shown to be geometric isomers whose derivation from the original tiglic and angelic acids can be shown. Since, however, the accuracy of the results obtained by Hölz[1] in his work upon the isomeric brompseudobutylenes, with which the bromine compounds of the pseudobutylenes obtained from these acids were compared, has been questioned by Faworsky and Debout,[2] a confirmation of the results of these experiments is necessary, and the question of the identity of the decomposition products from the iodohydrotiglic and the iodohydroangelic acids must remain open for the present.

[1] Liebig's Annalen, 250, 224. [2] Journal für praktische Chemie, 42, 149.

TECHNOLOGY QUARTERLY.

VOL. IV. OCTOBER, 1891. NO. 3.

AN EXPERIMENTAL STUDY OF THE WASTE FIELD OF DYNAMOS.[1]

BY WM. L. PUFFER, INSTRUCTOR IN PHYSICS.

IN 1886 a remarkable paper was published by Drs. John and Edward Hopkinson concerning the magnetic circuit of certain types of dynamos. In this paper were given some measurements of the quantity called "v," or the ratio of the total flux of force in the field magnet to that in the armature; this was found to be about 1.32.

The method used was the ballistic galvanometer one of measuring induction through a closed magnetic circuit, the dynamo magnet being so regarded. Turns of wire were placed around the field coils, armature, and other parts of the machine, as well as in the air about the magnet. The published report states that the magnets were alternately excited and short circuited; the terminals of the secondary coils were connected in turn to a reflecting galvanometer with a weighted needle.

In a paper read before the Institution of Electrical Engineers in February, 1890, Mr. W. B. Esson gave a table of values of "v" for different dynamos. This table will be quoted later in the paper. At my suggestion, Messrs. Brown and Greenlaw decided to make an extended and careful set of tests on a number of forms of dynamo magnets which were in the dynamo room of the Physical Department. After a few experimental trials, the apparatus and circuits were

[1] I have embodied in this paper the results of the graduating thesis of Messrs. Edward D. Brown and Frank M. Greenlaw, of the class of 1890, Massachusetts Institute of Technology, together with some of my own measurements made in the winter of 1887–88.

arranged in the following manner: All observations were taken in a basement room remote from the electrical and mechanical disturbances of the dynamos. A Hartmann galvanometer was set up, with a wooden block substituted for the copper one in which the bell-magnet is usually placed. The coils used with the instrument were of about 356 ohms, connected in series; an adjustable resistance was used to control the extent of the deflection without change of adjustment of galvanometer coils. This resistance was kept constant during a test. The deflections were read by a telescope and scale. A few turns of wire were wound over the regular coils and put in circuit with a suitable resistance, a cell of battery and reversing key, to enable the observer to quickly bring the needle to rest after a deflection had been received.

Exploring coils were wound upon different parts of the dynamo to be tested, the number of turns of each coil was chosen so that the deflections of the ballistic galvanometer should in all cases be as nearly alike as convenient. A mercury-cup switch-board was arranged with a set of cups for the terminals of each coil, and an ebonite block with two pins in it was arranged with flexible twisted wires, so that the galvanometer could be quickly connected to any one of the exploring coils.

The dynamo experimented upon was necessarily at rest, and the fields excited by another machine; the current in the magnet coils was of the strength it should be if the machine were running properly at no load. Of course the values of "v" thus obtained are not correct for the machine when running either at partial or full load.

The method of conducting the experiments was for the observer at the galvanometer to signal by telegraph to the operator at the dynamo his readiness to receive a deflection, at which the field circuit was closed; on again receiving a signal the circuit was opened. The same readings were then taken with the galvanometer commutator reversed, and in turn the procedure was repeated for all the coils upon the machine. Several series of deflections were taken at each time of experimenting, and from these means the results were calculated.

The Edison dynamo was first chosen on account of the simplicity of its construction. It is rated as a No. 4 dynamo, having a capacity of 150 110 volt, 16 candle-power, incandescent lamps, and as the lamps

furnished with the dynamo average about 50 watts each, this is equivalent to rating the machine at 7,500 watts; but as the ammeter is marked full load at 80 ampères corresponding to 8,800 watts, the exact rating is doubtful. The dynamo will give an output of 90 ampères all day and run cool and give no trouble.

DIMENSIONS.

Taken from a blue print.

Wrought-iron magnet cores . . .	7⅞ inches in diameter, 12½ inches long.
Wrought-iron yoke	10⅛ inches long, 5⅛ inches wide and thick.
Cast-iron pole pieces	12 inches long, 7⅞ inches high.
Clearance	1/16 inch.
Horns of pole pieces	2¾ inches apart.
Armature core washers	6½ inches in diameter, with 2⅜ inch hole.
Armature core	12 inches long, 6½ inches in diameter.
Revolutions per minute	1520.

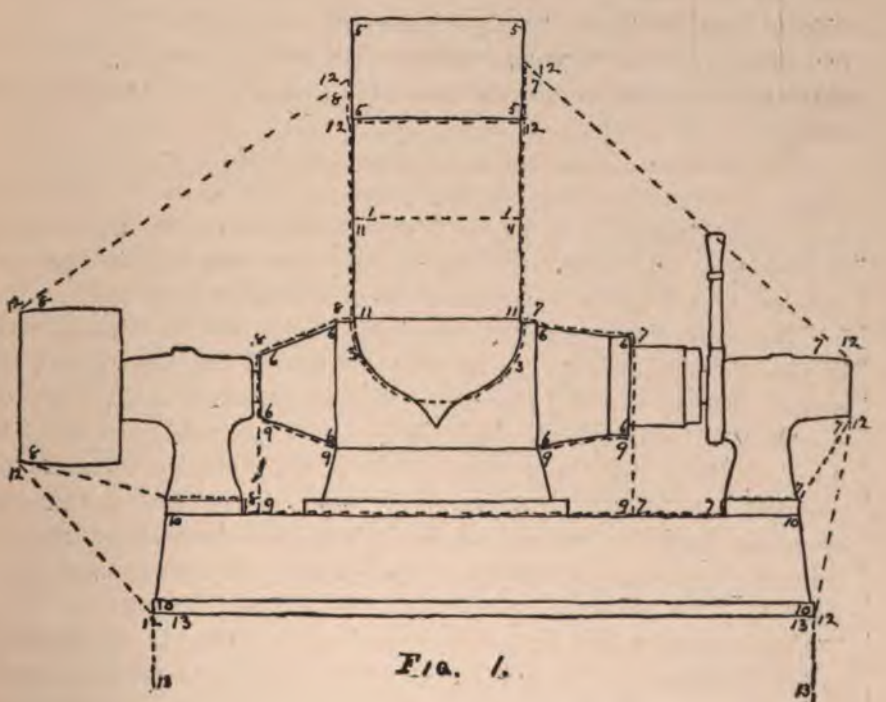

FIG. 1.

Figure 1 will show better than words the position of the various exploring coils which will be named as follows:

EXPLORING COILS.

Coil.	Turns.	Position.
1	1	Middle of magnet spool.
2	1	Middle of magnet spool.
3	1	Pole piece.
4	1	Pole piece.
5	1	Middle of yoke.
6	1	Armature, including heads.
7	6	Space, including commutator and bearing.
8	6	Space, including pulley and bearing.
9	10	Space between armature and iron base.
10	5	Iron bed-plate.
11	12	Between magnet coils from armature to plane of 1 and 2.
12	1	Whole machine and bed-plate, except yoke.
13	5	Under bed-plate to a depth of about 14 inches.

Two methods were used to produce the magnetization and demagnetization of the machine, which I will call the first method and the second method respectively. In the first method the field magnet circuit was completed and broken by means of the main switch of the dynamo. A considerable arc was formed upon breaking, which was at first reduced by connecting the pilot lamp across the magnet terminals; but as this reduced the deflection, its use was given up and the arc allowed to burn. This method is objectionable on account of the great strain the insulation of the wire has to stand, and would not be safe to use on high voltage machines. The second method was devised to put less strain on the insulation, as well as to find what the difference would be between it and the first method. The magnet was connected in series with a suitable resistance to mains of higher voltage (180 volts), and a snap-switch arranged to demagnetize the machine by short-circuiting it, and to magnetize it when the short circuit was opened. This method worked very well and was perfectly satisfactory

in every way. It was found that when the machine was magnetized, these methods gave practically the same results, both in the value of the deflections and in the percentage distribution of the lines of force; but when the machine was demagnetized, the deflections of the first method were slightly more than twice as great as in the second, although the percentage values were not so widely different.

The table shows the mean percentage distribution of the magnetization as found by both the first and second methods, and also the mean of these; one hundred per cent. represents the mean value of the magnetization of the field coils.

EDISON INCANDESCENT DYNAMO.

Percentage Distribution of Magnetization.

Coil.	Position of Coil.	First Method.		Second Method.		Average.	
		Mag.	Demag.	Mag.	Demag.	Mag.	Demag.
1 & 2	Middle of magnetizing coils . . .	100.	100.	100.	100.	100.	100.
3 & 4	Pole pieces	86.8	84.0	86.0	83.1	86.4	83.6
5	Middle of yoke	98.3	96.3	96.5	97.3	97.4	96.8
6	Armature, including heads . . .	65.8	63.5	64.8	63.7	65.3	63.6
7	Space, including commutator and bearing	2.3	2.3	2.6	2.6	2.5	2.5
8	Space, including pulley and bearing .	2.9	3.0	2.9	3.3	2.9	3.2
9	Space between armature and bed-plate	3.8	3.6	3.8	3.7	3.8	3.7
10	Iron bed-plate	9.3	8.8	9.2	8.3	9.3	8.6
11	Between magnet coils	5.6	6.0	5.6	6.4	5.6	6.2
12	Whole machine and bed, except yoke	—	—	90.1	90.5	—	—
13	Wood frame under bed-plate . . .	—	—	2.6	2.9	—	—
	The value of "v" is	1.52	1.57	1.54	1.57	1.53	1.57

I am not able to satisfactorily explain the cause of variation between the two methods or between the makes and breaks, but I am inclined to think it is due mostly to the fact that the ballistic method assumes

one condition which cannot hold with large magnetic circuits, and that is that the needle receives the impulse before it has moved appreciably. Probably these discrepancies would not be observed if a galvanometer should be made with a specially constructed needle of great mass, built up of thin laminæ so arranged that the poles of the magnetic system would not be altered by the current in the coils of the instrument.

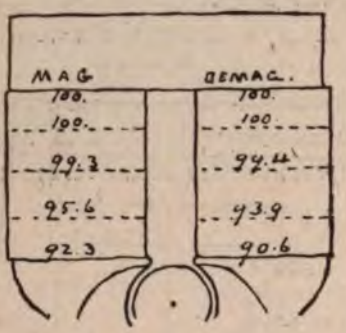

FIG. 2.

The leakage along the magnet limbs is shown by Fig. 2, which gives the positions in which the exploring coil was placed, and the values obtained.

THOMSON-HOUSTON INCANDESCENT DYNAMO.

This dynamo is of very peculiar design, being in the main an adaptation of the well-known Thomson-Houston arc-light pattern to an incandescent machine. The spherical armature revolves between the cup-shaped ends of two horizontal cylindrical cores, each of which has a heavy flange on its outer end. These flanges are connected by sixteen two-inch wrought-iron bars, as well as by two heavy cast-iron side brackets which support the main bearings of the armature shaft, and have downward projections to serve as the four legs upon which the machine stands.

As there is very little external evidence of magnetism when the dynamo is running, and the yoke bars and brackets are in effect a closed cylindrical iron shell, with its axis concentric with the axis of the magnet cores, preliminary examination was made with a small compass, and the general direction of the leakage determined. These leakage lines were substantially like those given in Hering's "Principles of Dynamo Electric Machinery."

Strong consequent points were found in the yoke bars, and a number of trials had to be made before a satisfactory set of exploring coils could be arranged. This dynamo was presented to the Physical Department with the understanding that its rating was 9,600 watts at a speed of 1,250 revolutions per minute, and a voltage of 110; but the same size

machine was listed as a 200 light dynamo with an output of 12,600 watts. It will easily run all day delivering 150 ampères at 110 volts, or 16,500 watts, without any undue heating or trouble of any kind. Exploring coils were arranged as shown in Fig. 3.

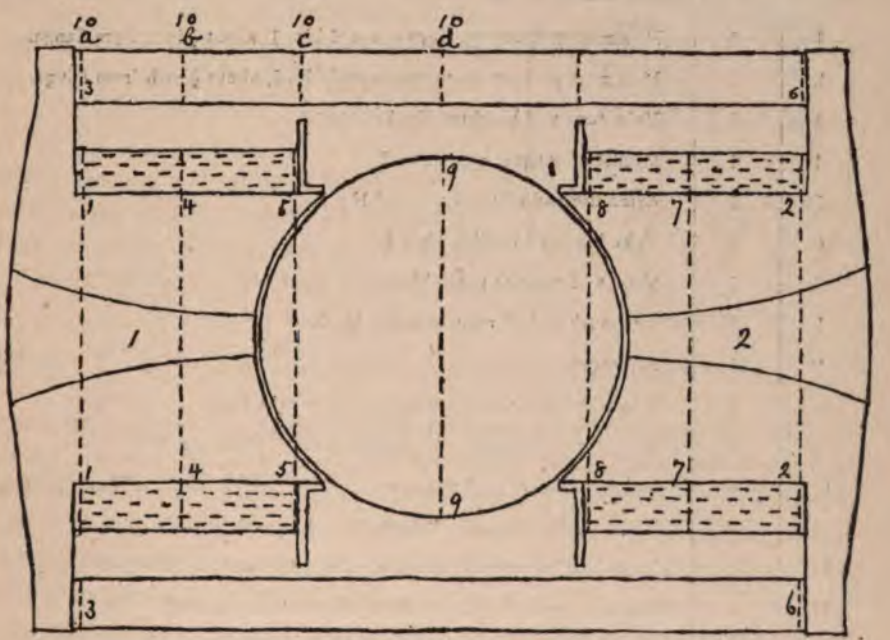

FIG. 3.

A small compass was used in determining the proper positions for these coils; very little magnetism was evident outside of the machine, but there was a very strong field between the poles and the yoke bars. The method of making and breaking the field circuit was alone employed on this dynamo, as well as on all other machines tested.

The table shows the percentage distribution of the magnetization as found both by magnetizing and demagnetizing. Coil 2 is lower than 1, and 8 than 5, probably because of the German silver turns which were short-circuited for regulation of potential of the dynamo; in this machine the resistance is wound on the machine itself and cut out when necessary by suitable screws and slides on the top of the magnet spools.

EXPLORING COILS.

Coil.	Turns.	Position.
1	1	Maximum position on magnet spool No. 1, about ½ inch from flange.
2	1	Maximum position on magnet spool No. 2, about ½ inch from flange.
3	1	Yoke bars and brackets No. 1.
4	1	Middle of magnet spool No. 1.
5	1	Armature end of magnet spool No. 1.
6	1	Yoke bars and brackets No. 2.
7	1	Middle of magnet spool No. 2.
8	1	Armature end of magnet spool No. 2.
9	1	Armature.
10	5	Moveable coils around one group of 4 yoke bars.
11	5	End of yoke bracket. Commutator side. Plane of 10 a.
12	6	Yoke bracket opposite magnetic pole. Commutator side. Plane of 10 c.
13	6	End of yoke bracket. Pulley side. Plane of 10 a.
14	6	Yoke bracket opposite magnet pole. Pulley side. Plane of 10 c.
15	6	Middle of yoke bracket. Pulley side. Plane of 10 d.

Although these results are of value in the study of dynamo magnets, it must be remembered that this machine was at rest, and there was no current in the armature or series winding. The series turns are placed over the armature and at such a fixed inclination as to tend to neutralize the cross magnetization of the armature, rendering the frequent adjustment of the brushes unnecessary. These series coils are very powerful and must make a very decided difference in the distribution of magnetism.

THOMSON–HOUSTON INCANDESCENT DYNAMO.

Percentage Distribution of Magnetization.

Coil.	Position of Coil.	Mag.	Demag.
1	Maximum position, spool No. 1	100.0	100.0
2	Maximum position, spool No. 2	98.7	99.1
3	Yoke bars and brackets No. 1	94.3	93.5
4	Middle of spool No. 1	95.5	96.6
5	Armature end of spool No. 1	81.5	82.5
6	Yoke bars and brackets No. 2	93.5	92.2
7	Middle of spool No. 2	95.3	96.2
8	Armature end of spool No. 2	80.3	79.9
9	Armature	70.6	71.1
10 a	Yoke bars	18.8	18.2
10 b	Yoke bars	18.1	17.1
10 c	Yoke bars	15.6	15.2
10 d	Yoke bars	14.2	13.6
11	End of yoke bracket, commutator side	8.7	8.6
12	Yoke bracket opposite pole, commutator side	5.7	5.4
13	End of yoke bracket, pulley side	12.1	11.4
14	Yoke bracket opposite pole, pulley side	9.1	8.1
15	Middle of yoke bracket, pulley side	7.7	6.9
	The value of "v" is	1.42	1.40

THE WESTON INCANDESCENT DYNAMO.

This dynamo is of the well-known double magnetic circuit form, made to run 60 70 volt 16 candle-power lamps. The magnet spools, four in number, are placed horizontally and wound on wrought-iron elliptical cores 8 inches long with axes about 5 and 11 inches. The yoke pieces are of cast iron, practically rectangular in cross section, with an area of about 38 square inches. The two pole-pieces are also of cast iron, with a section of about 90 square inches. The total

length of a single magnetic circuit is about 45 inches. Figure 4 shows the positions of the exploring coils used. The composition bearing at the pulley end of the shaft is held by arms which project from the upper and lower pole piece, and come so near together that it would seem as if together with the shaft a very good path was available for stray lines of force. Coil 7 was wound upon one of the iron supports, and is not shown on the figure.

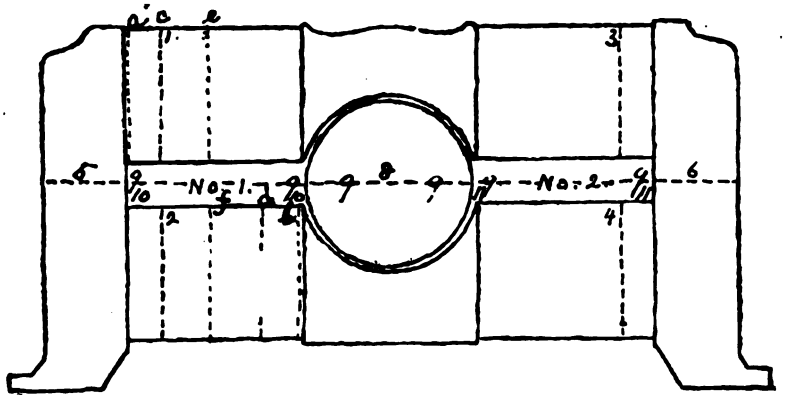

FIG. 4.

EXPLORING COILS.

Coil.	Turns.	Position.
1	1	Upper magnet spool No. 1, two inches from yoke.
2	1	Lower magnet spool No. 1, two inches from yoke.
3	1	Upper magnet spool No. 2, two inches from yoke.
4	1	Lower magnet spool No. 2, two inches from yoke.
5	1	Yoke No. 1.
6	1	Yoke No. 2.
7	10	Pulley bearing arm.
8	1	Armature.
9	1	Whole plane.
10	10	Between spools magnet No. 1.
11	10	Between spools magnet No. 2.

One half of the sum of the deflection from coils 1, 2, 3, 4, is taken as 100 per cent. in the table of percentage distribution. The four values were almost identical, indicating a very good balance of the windings.

WESTON INCANDESCENT DYNAMO.

Percentage Distribution of Magnetization.

Coil.	Position of Coil.	Mag.	Demag.
1 & 2	Upper and lower magnet spool No. 1	100.0	100.0
3 & 4	Upper and lower magnet spool No. 2	100.0	100.0
5	Yoke No. 1	93.3	93.0
6	Yoke No. 2	93.3	93.0
7	Pulley bearing arm	3.7	3.7
8	Armature	78.9	78.8
9	Whole plane	91.8	93.0
10	Between spools No. 1	3.3	3.3
11	Between spools No. 2	3.4	3.4
1a	. . . . .	98.5	98.5
1c	. . . . .	100.0	100.0
1e	. . . . .	98.5	98.5
2f	. . . . .	99.0	98.5
2d	. . . . .	95.8	95.9
2b	. . . . .	91.5	92.4
	The value of "v" is	1.27	1.27

THOMSON–HOUSTON MOTOR.

This motor has magnets of the inverted type of single magnetic circuit; the yoke is cast in one piece with the bed-plate. The armature bearings are supported by iron pillars from the bed-plate. The vertical magnet cores are of circular section about $7\frac{1}{2}$ inches in diameter and $8\frac{3}{4}$ inches long. The pole pieces are 10 inches long and $8\frac{1}{2}$ inches high. As this machine was in almost constant use, only a few

measurements could be taken, and are given only as interesting in connection with this paper, and not as the result of a careful and complete study of the waste field.

Coil 5 was arranged to see if there was a great leakage between the horns of the pole pieces, which appear to be very close together.

THOMSON–HOUSTON 220 VOLT, 10 H.P. MOTOR.

Percentage Distribution of Magnetization.

Coil.	Position of Coil.	Mag.	Demag.
1	Magnet spool 2 inches from yoke. Maximum	100.0	100.0
2	Magnet spool at yoke	99.0	99.6
3	Magnet spool at pole piece	91.4	92.2
4	Armature	75.9	75.1
5	Around armature, 1 inch from it	78.1	78.6
	The value of "v" is	1.32	1.33

THE EDDY DYNAMO.

This machine was a special one made for the Mining Department of the Massachusetts Institute of Technology for a maximum output of 50 ampères at 50 volts. The old style Eddy motor frame was used in this machine, and at the request of the Eddy Company tests were made of magnetic leakages, efficiencies, etc., both with and without certain iron and composition parts. The object was to learn what could be done to improve the design of the machine, and incidentally to find out just what difference would be made by the substitution of some non-magnetic metal for iron in the construction of the bearing supports and pulley.

The method of studying the magnetic losses was substantially the same as already described, except that the results were calculated from the mean of the deflections produced by magnetizing and demagnetizing.

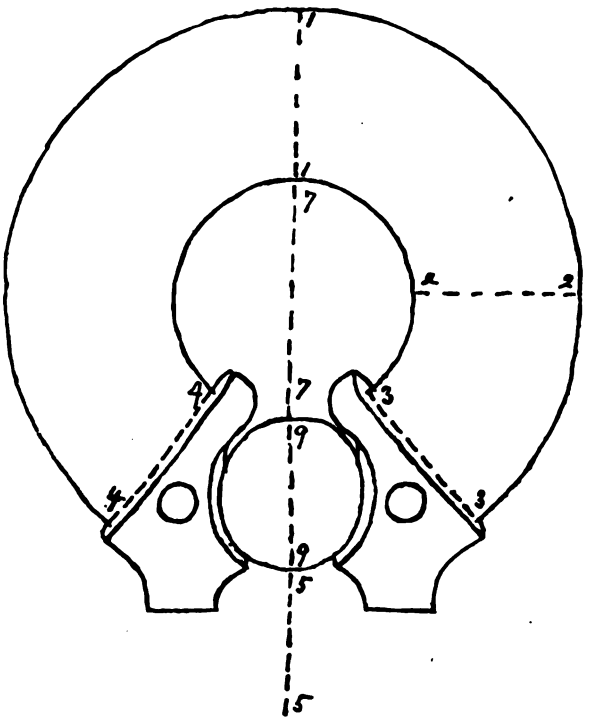

FIG. 5.

EXPLORING COILS.

Coil.	Position.
1	Top of magnet.
2	Side of magnet.
3	Side of magnet close to pole-piece.
4	Side of magnet close to pole piece.
5	Space between armature and floor.
6	Space, including commutator and bearing.
7	Between magnet coils.
8	Space, including pulley and bearing.
9	Armature.

The "parts" mentioned consisted of the pulley and four large pins which screwed into the pole pieces and furnished supports for the composition bearing pieces. With the iron parts in place, there was a partial magnetic circuit through the two pulley bearing pins and the pulley, and also through the two commutator bearing pins, and the iron rocker-arm that carried the brushes.

EDDY DYNAMO.

Percentage Distribution of Magnetization.

Coil.	Position.	Composition.	Iron.
1	Top of magnet	100.0	100.0
2	Side of magnet	95.0	95.6
3	Pole piece	88.1	88.9
4	Pole piece	88.5	89.4
5	Base	8.46	7.24
6	Commutator side	2.23	5.43
7	Between magnet coils	6.23	5.68
8	Pulley side	2.60	3.98
9	Armatures	66.9	61.4
	The value of "v" is	1.49	1.63

It is quite evident that with the iron parts in place there is less leakage of lines of force from the sides of the magnet, in the space over the armature and under the machine; but that the loss at the commutator side is increased about 100%, and at the pulley side about 65%, causing a loss of 8.8% in the armature. The change from iron to composition parts increased the commercial efficiency of the machine by about 1.6%, not however, so much by a saving in the energy spent in the magnet as by reducing the eddy currents in the iron pulley which became very perceptibly hot while running. Since these tests were made, this machine has been very greatly improved.

The following table gives the values of "v" as found by W. B. Esson, together with the values given in this article:

Machine.	Field.	Armature.	Remarks.	"v."
Edison-Hopkinson	Single magnet.	Drum . .	Poles next bed-plate . . .	1.32
Siemens	Single magnet.	Drum . .	Yoke next bed-plate . . .	1.30
Phœnix	Single magnet.	Cylinder .	Yoke next bed-plate . . .	1.32
Phœnix	Double magnet	Cylinder .	Horizontal	1.40
Manchester . . .	Double magnet	Cylinder .	Bed and one pole common .	1.49
Victoria	Double magnet	Ring . .	Ordinary pattern	1.40
Ferranti	Double magnet	Coreless .	Alternating	2.00
Edison	Single magnet.	Drum . .	. . . . .	1.53–1.57
Thomson-Houston	Single magnet.	Sphere .	. . . . .	1.42–1.40
Weston	Double magnet	Drum . .	. . . . .	1.27–1.27
Thomson-Houston motor type . .	Single magnet.	Drum . .	. . . . .	1.32–1.33
Eddy	Single magnet.	Drum . .	Composition parts . . .	1.49
Eddy	Single magnet.	Drum . .	Iron parts	1.63

ROGERS LABORATORY OF PHYSICS,
August, 1891.

THE DIRECT DETERMINATION OF ALUMINUM IN IRON AND STEEL.[1]

BY THOMAS M. DROWN AND ALEXANDER G. McKENNA.

THE unsatisfactory character of most, if not all, of the processes for the direct determination of aluminum in the presence of iron and phosphoric acid, and the sharpness with which both the iron and phosphoric acid can be determined, have led chemists to rely mainly on processes in which the aluminum is obtained by difference. But in the modern alloys of aluminum and iron, where the aluminum may be present only to the extent of a small fraction of one per cent., nothing short of the isolation of the aluminum itself can give satisfactory evidence of its presence.

In the course of some experiments on the electrolytic deposition of metals we have found that it is possible to remove iron completely from its acid solution in large quantity and in short time, by a current that will not in the least affect the aluminum in solution. The essential condition of success in this operation is the use of mercury as a cathode, the iron forming an amalgam with the mercury as fast as it is deposited from solution. This use of mercury was proposed by Dr. Wolcott Gibbs, in a paper read before the National Academy of Science in 1883, as a method of determining many metals by electrolysis which are not at all, or only imperfectly, deposited on a platinum cathode. It is certainly one of the most valuable and suggestive contributions to the quantitative separation and determination of metals by electrolysis that has yet been made.

The following are some of the experiments we have made to determine the conditions of success in the application of the method to the determination of aluminum in the presence of iron:

First, as to the completeness of the precipitation of the iron. The

[1] Abstract from Thesis, Course V, 1891, read at the Cleveland meeting, American Institute of Mining Engineers, June, 1891.

first attempts to determine the iron which had been removed from the solution and alloyed with the mercury showed an apparent loss, notwithstanding the fact that the iron was completely removed from solution. This was subsequently found to be due to the volatilization of some of the mercury on drying before weighing. The amount of mercury which may be thus lost is very considerable, as the following experiments show.

About 75 grams of mercury were put into a beaker, which was then weighed. It was washed several times with water, and once or twice with alcohol. The loss on drying different times and at different temperatures was as follows:

Time of Drying.	Temperature.	Loss, Gram.
10 minutes	140° C.	.0096
10 minutes	100°	.0043
10 minutes	100°	.0028
10 minutes	100°	.0030
10 minutes	100°	.0030
10 minutes	100°	.0025
2 hours	70°	.0040
5 hours	70°	.0120
20 hours	70°	.0520
48 hours	25°	.0030
48 hours	24°	.0025
24 hours	24°	.0007
24 hours	24°	.0012
48 hours	24°	.0042
48 hours	24°	.0033

But it is, in fact, only necessary to heat the mercury for about two minutes at a temperature of 100° C. to free it completely from moisture, and in this time the loss is very small. The following are the amounts lost in nine experiments under these conditions: .0002, .0005, .0005, .0003, .0008, .0005, .0002, 0, 0, — an average of 0.00033 gram.

There seems also, at times, to be a loss not connected with the volatilization of the mercury, which, possibly, may be due to impuri-

ties in the mercury, metals, for instance, which are dissolved out by the acid, and not precipitated on the mercury again. On this account it is desirable, in cases where the process is used to determine iron or other metal, to have a blank beaker in the circuit, containing only the mercury and dilute sulphuric acid, and to add any loss which may be found in this beaker to the amount found in the others in the series.

The following are a series of results obtained by electrolyzing a solution of ferrous ammonium sulphate, slightly acidulated with sulphuric acid.

Iron Taken. Gram.	Iron Found. Gram.	Loss of Weight in Blank. Gram.	Iron Found + Loss in Blank. Gram.
.2260	.2254	Not made.	. . .
.3727	.3725	Not made.	. . .
.3080	.3089	Not made.	. . .
.3238	.3232	Not made.	. . .
.0647	.0632	.0020	.0652
.0647	.0634	Not made.	. . .
.0647	.0649	0000	.0649
.0647	.0635	Not made.	. . .
.0604	.0602	.0003	.0605
.0604	.0608	.0003	.0611
.3020	.3008	.0003	.3011
.3020	.3000	.0003	.3003

The best conditions for the rapid precipitation of the iron are a strong current (about 2 ampères or 20 cc. of electrolytic gas a minute), a nearly neutral solution of small bulk, a large amount of mercury (not less than fifty times the weight of the iron to be precipitated), and a large anode of platinum. The mercury cathode is brought into the circuit by means of a platinum wire inclosed and fused into one end of a glass tube, which passes through the liquid. It is of advantage to pour mercury into this tube to the depth of an inch, in order to weight it and make the connection with the mercury cathode more stable.

By observing the above conditions it was found practicable to precipitate 10 grams of iron in from 10 to 15 hours. The iron amalgam may contain as high as 10 per cent. of iron. On standing a long time exposed to the air, the iron separates out as a black powder, which may be removed from the mercury by agitation with water. Mercury which has been used for iron precipitations may be purified by removing the greater part of the iron amalgam by filtration through chamois skin, and then passing air through the filtered portion, as recommended by J. M. Crafts.

Second, as to the behavior of manganese in solution under the foregoing conditions. Inasmuch as manganese is almost always present in iron and steel, a good many experiments were made to find what becomes of it under the electrolytic conditions which we have been considering. It was found that a portion is oxidized to the dioxide, coating the platinum anode, and ultimately falling off, in great part, in scales, and that a portion is reduced to the metallic state, and alloys with the mercury. This fact of the reduction of the manganese was evident on electrolyzing a solution containing only dilute sulphuric acid, using as a cathode mercury which had been previously used in an experiment with a manganese solution. In a few minutes the permanganate color appeared, and a slight coating of manganese dioxide was deposited on the anode. The experiments made to determine what proportion of manganese was reduced and what proportion oxidized were not satisfactory, but in general it may be said that about half the manganese alloys with the mercury; the other half is nearly all oxidized, and is found both on the anode and in scales floating in the liquid. A small portion is always found in solution. By using both electrodes of platinum it was found possible, in the absence of iron, to precipitate manganese completely as dioxide; but the coating on the anode was never coherent when more than mere traces of manganese were present. But the danger of the re-solution of the floating particles of the manganese dioxide, together with the necessity of filtering and igniting it to the proto-sesquioxide, makes the process an unsatisfactory one for the determination of manganese. The best conditions were found to be a concentrated solution of manganese sulphate to which a little nitric acid had been added, a rather weak current, a small cathode, and a large anode.

Third, as to the influence of phosphoric acid on the precipitation of iron. Two solutions containing equal amounts of iron were elec-

trolyzed in series. To one was added one gram of crystallized hydrogen di-sodium phosphate, and to the other dilute sulphuric acid. When the iron was all precipitated, the beakers with the mercury, together with the glass tubes containing the platinum wires, were weighed, and found to have gained, respectively, 0.1290 and 0.1275 gram. To a solution of 3 grams of iron in sulphuric acid, 0.1935 gram crystallized hydrogen di-sodium phosphate was added, equivalent to .0167 phosphorus. After the iron was completely removed from the solution, the phosphoric acid was precipitated by magnesium mixture, and .0163 gram phosphorus obtained. A duplicate experiment gave .0161 gram phosphorus. A third experiment with the same amount of iron gave, under like conditions, .0064 gram phosphorus when .0068 was added. From the experiments we may conclude that the separation, by this electrolytic method, of iron and phosphoric acid in solution is satisfactory.

Fourth, as to the separation of iron and aluminum. Known amounts of iron and aluminum in solution together as sulphates were electrolyzed, and, after all the iron had been deposited, the alumina was precipitated by ammonia. Following are two results thus obtained:

Alumina Taken. Gram.	Alumina Found. Gram.	Iron Taken. Gram.	Iron Found. Gram.
.0283	.0286	.2286	.2277
.0142	.0142	.2286	.2283

The iron, determined by weighing the mercury after electrolyzing the solution, includes in both cases a blank of .0020 gram, which was the loss of a mercury cathode in the same series, when dilute sulphuric acid only was electrolyzed.

The process was now tried under the conditions which would obtain in the determination of aluminum in iron and steel, namely, known amounts of aluminum sulphate were added to a known amount of steel. After much experimenting the following procedure was adopted as being the most uniformly satisfactory.

Dissolve 5 to 10 grams of iron or steel in sulphuric acid, evaporate until white fumes of sulphuric anhydride begin to come off, add water, heat until all the iron is in solution, filter off the silica and carbon, and wash with water acidulated with sulphuric acid. Make the filtrate nearly neutral with ammonia, and add to the beaker, in which the electrolysis is to be made, about one hundred

times as much mercury as the weight of iron or steel taken. The bulk of the solution should be from 300 to 500 cc. Connect with the battery or dynamo current in such a way that about two amperes may pass through the solution over night. This we have generally accomplished by using three lamps of 32 candle-power arranged in parallel on an Edison circuit. In the morning the solution is tested for iron, and, if necessary, the electrolysis is continued after adding enough ammonia to neutralize the acid that has been set free by the deposition of the iron. The progress of the operation may be observed by the changing color of the solution. At first it becomes darker in color near the anode; after five or six hours it is nearly colorless, and finally becomes pink, from the formation of permanganate.

When the solution gives no test for iron, it is removed from the beaker with a pipette while the current is still passing. When as much has been removed as possible without breaking the current, water is added, and the operation continued until the acid has been so far diluted that there is no danger of dissolving iron from the mercury. The anode is now taken out, and the mercury washed with water until the last traces of the solution have been removed from it. After filtering, to remove any flakes of manganese dioxide which may be suspended in the solution, sodium phosphate is added in excess, and 10 grams of sodium acetate. The solution is now made nearly neutral with ammonia, and boiled for not less than forty minutes. The precipitate of aluminum phosphate is then filtered off, ignited, and weighed. It should be white after ignition. If it has more than the faintest shade of color it must be dissolved by fusing with acid potassium sulphate, and again electrolyzed for two or three hours. The second precipitate has been found to be always white without a trace of iron; but, in general, we have found the first precipitate to be free from iron, or to contain such a minute amount that the second treatment is not necessary. We have determined that .0003 gram of iron will give a decided red color to .0076 gram of aluminum phosphate. The precipitate of aluminum phosphate, produced as above, does not always have the composition $Al_2O_3P_2O_5$. Our analyses of it go to show that the formula $7Al_2O_36P_2O_5$ more nearly expresses its average composition, and in those cases in which the amount of phosphoric acid and alumina have not been actually determined in the precipitate, we have assumed this composition, which is equivalent to 24.14 per cent. of aluminum in the precipitate.

It is necessary to boil for at least forty minutes to precipitate completely the aluminum phosphate, and excessive washing of the precipitate should be avoided.

The following table gives the result obtained in determining by the above process the aluminum added in known amounts to solutions of steel:

Steel Taken. Grams.	Per cent. of Aluminum added.	Per cent. of Aluminum found.
5	0.39	0.36
5	0.39	0.38
5	0.39	0.38
5	0.39	0.38
5	0.39	0.37
5	0.043	0.045
5	0.043	0.041
5	0.043	0.049
5	0.043	0.048
10	0.027	0.015
10	0.200	0.160
10	0.046	0.044
5	0.085	0.088

A blank experiment with the same steel, without the addition of any aluminum, gave a precipitate of aluminum phosphate equivalent to 0.004 per cent. of aluminum, which has been deducted from the results in the above determinations. This small amount came, doubtless, from the reagents and the glass vessels used, and may have been partly silica. This correction should be made in all cases where the amount of aluminum is very small.

A sample of Mitis steel analyzed by this method gave the following result:

Steel Taken. Grams.	Aluminum Found. Per Cent.
4.4260	0.014
4.4260	0.013
12.6650	0.014

A sample of "aluminum physic" used in the preparation of Mitis castings gave as follows :

Iron Taken. Grams.	Aluminum Found. Per Cent.
1.6130	6.28
0.4030	6.52
0.4030	6.22

A sample of pig-iron to which aluminum had been added in remelting gave as follows :

Iron Taken. Grams.	Aluminum. Per Cent.
1.4576	0.789
1.8167	0.806
1.6390	0.794
1.6130	0.802

It might be thought that the process would be simplified by reducing the iron to the state of protoxide, and then precipitating the alumina as basic acetate, subsequently removing by electrolysis the small amount of iron precipitated with the alumina. A number of experiments proved, however, that this modification not only gave less accurate results, but involved much more work than the precipitation of all the iron by electrolysis. When the iron is all removed over night without involving any work on the part of the analyst, it is surely superfluous to go through a more or less tedious chemical operation to remove a part of the iron.

It is obvious that this process of electrolysis with a mercury cathode may be of value in other analytical operations in which the removal of the iron would be of advantage; as, for instance, in the determination of phosphoric acid by magnesia.

CONTRIBUTION TO OUR KNOWLEDGE OF ORTHODICHLORSTILBENE.[1]

BY AUGUSTUS H. GILL, PH.D.

INTRODUCTION: Onufrowicz[2] obtained by the action of finely divided copper upon benzalchloride, stilbene chloride, according to the equation: —

$$2C_6H_5\,CHCl_2 + 2Cu = C_6H_5\,CHCl - CHCl\,C_6H_5 + 2CuCl.$$

If, however, the compound in which one of the hydrogen atoms in the ring had been substituted by chlorine — orthochlorbenzalchloride — was employed, similar results were to be expected, and orthodichlorstilbene chloride would be formed: —

$$2C_6H_4{<}^{Cl}_{CHCl_2} + 2Cu = C_6H_4{<}^{Cl}_{CHCl} - {}^{Cl}_{ClHC}{>}C_6H_4 + 2CuCl.$$

If this compound be treated with an insufficient quantity of alcoholic potash, as stilbene dichloride was treated by Laurent,[3] it is to be expected that only one molecule of hydrochloric acid would be split off, and orthodichlortolane hydrochloride result: —

$$C_6H_4{<}^{Cl}_{CHCl} - {}^{Cl}_{ClCH}{>}C_6H_4 + KOH = C_6H_4{<}^{Cl}_{CH} = {}^{Cl}_{ClC}{>}C_6H_4 + KCl + H_2O.$$

If a large excess of alcoholic potash be present, then the reaction would take place, according to Fittig,[4] with the formation of orthodichlortolane: —

$$C_6H_4{<}^{Cl}_{CHCl} - {}^{Cl}_{ClCH}{>}C_6H_4 + 2KOH = C_6H_4{<}^{Cl}_{C} \equiv {}^{Cl}_{C}{>}C_6H_4 + 2KCl + 2H_2O.$$

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[1] Inaugural Dissertation presented to the Philosophical Faculty of the University of Leipsic for the degree of Doctor of Philosophy, July, 1890.

[2] Berichte der deutschen chemischen Gesellschaft, 17, 835.

[3] Berzelius' Jahresbericht, 25, 616. [4] Liebig's Annalen, 168, 74.

If zinc dust be substituted for potash, it should remove two atoms of chlorine, and dichlorstilbene should be formed: —

$$C_6H_4 < \begin{matrix} Cl \\ CHCl \end{matrix} - \begin{matrix} Cl \\ ClHC \end{matrix} > C_6H_4 + Zn = C_6H_4 < \begin{matrix} Cl \\ CH = \end{matrix} \quad \begin{matrix} Cl \\ = HC \end{matrix} > C_6H_4 + ZnCl_2.$$

Were it then possible to remove the other chlorine atoms from this compound by the use of sodium or potassium, a new synthesis of phenanthrene would be accomplished: —

$$C_6H_4 < \begin{matrix} Cl \\ CH = \end{matrix} \quad \begin{matrix} Cl \\ = HC \end{matrix} > C_6H_4 + 2Na = \overline{C_6H_4 - CH = CH - C_6H_4} + 2NaCl.$$

This line of thought was suggested to me as a subject for a thesis by Professor Johannes Wislicenus, of the University of Leipsic. The following pages contain the results of the investigation.

PREPARATION OF ORTHOCHLORBENZALCHLORIDE.

According to Henry[1] this compound is readily obtained by the action of phosphorus pentachloride upon salicyl aldehyde; but the resulting oxychloride and excess of pentachloride are not easily separated from the product. To avoid this difficulty, orthochlortoluol was prepared, and this chlorinated at its boiling point. Its preparation took place as follows: 100 grams orthotoluidine were gradually poured into a mixture of 200 cc. hydrochloric acid (sp. gr. 1.16) and 300 cc. water, the resulting chloride not being wholly dissolved. To this was gradually added a solution of 90 grams 86% sodium nitrite in 150 cc. of water, the temperature being kept below 10°. Usually during the addition of the last portion of the nitrite solution there was a slight evolution of nitrogen, hence it is advisable to have an excess of hydrochloric acid present; the diazo body resulting remained dissolved. The mixture was allowed to stand half an hour to complete the reaction and then gradually poured into 1800 cc. of cooled hydrochloric acid (sp. gr. 1.16). The hydrochloric acid must be completely saturated. The stronger it is the better the yields obtained. An experiment with commercial acid gave only 30% of the toluidine used, when 50% was to be expected. The whole mixture was allowed to

[1] Berichte der deutschen chemischen Gesellschaft, 2, 135.

stand 18 – 24 hours, at first in a freezing mixture, and finally at ordinary temperature; it was then boiled, distilled with steam, and the oil which came over collected, washed with potassium hydrate to free it from the concomitant cresol, dried and fractionated. The portion boiling between 156° and 160° was used for chlorination; from 200 grams orthotoluidine 107 grams orthochlortoluol were usually obtained.

A steady stream of dry chlorine was passed into 100 grams orthochlortoluol heated to boiling until 50 – 55 grams were absorbed. Upon fractionation, a small quantity went over at 218°, the main portion which was used for further treatment between 226° and 230°. Upon repeated refractionation of this product, a colorless oily liquid was obtained, boiling at 228.5°, and possessing a specific gravity of 1.399 at 15°; 0.2144 gram of this compound gave 0.4724 gram AgCl, corresponding to 54.48% Cl;[1] the theory for $C_6H_4ClCHCl_2$ requires 54.77% Cl.

PREPARATION OF ORTHOCHLORBENZOTRICHLORIDE.

This body, which was first obtained by Kolbe and Lautermann[2] by the action of phosphorus pentachloride upon salicylic acid, could, as might be foreseen, be made by the further chlorination of orthochlorbenzalchloride. The high boiling residues left from the fractionation of the latter were treated with dry chlorine until saturated. The product distills between 257° and 259°, and forms under favorable conditions splendid monoclinic prisms, melting at 29°, which upon analysis yielded the following: 0.1545 gram substance gave 0.3869 gram AgCl, equivalent to 61.90% Cl. Theory for $C_6H_4ClCCl_3$, 61.70% Cl.

TREATMENT OF ORTHOCHLORBENZALCHLORIDE WITH METALS.

(1) *Copper.* Finely divided copper (*cuprum reductum*) in open vessels has no perceptible action upon the chloride; the amounts of each, the solvent, or the temperature, exercise no influence upon the result. Petroleum ethers (bpts. 55° and 120°), methyl and ethyl alcohol, benzol and toluol — liquids boiling at temperatures from 55° to 120° — were employed. A very small quantity of crystals of melting

[1] All chlorine determinations, unless it is stated to the contrary, were made according to the method of Carius.

[2] Liebig's Annalen, 115, 195.

point 97°, and also tar, were obtained, but the action is very slight; if, however, a great excess of copper is employed in sealed tubes, the yield of crystals is much increased.

(2) *Silver.* Molecular silver in alcoholic or naphtha solution (bpt. 110°) acts upon it with the formation of orthodichlorstilbene-dichloride, by the elimination of two atoms of chlorine; orthodichlorstilbene was also obtained in small quantities. Silver has a much more energetic action than copper in sealed tubes at 120° – 130°, with the formation of much tar and fewer crystals of orthodichlorstilbene.

(3) *Zinc.* Upon heating the chloride in naphtha solution (bpt. 110°) with carefully purified zinc dust for eight hours, very few crystals of orthodichlorstilbene resulted; a small quantity of tar was also formed.

(4) *Sodium.* No sodium chloride was formed upon heating the chloride with sodium wire in sealed tubes at 160° for eight hours.

It may be due to the fact that this is a disubstituted compound that the metals have so little action upon it; these bodies are apparently the most stable, as shown by the following facts. Let us consider the chloracetic acids: —

(a) *Monochloracetic acid.* It is slowly decomposed by water. Buchanan.[1]

(b) *Dichloracetic acid.* It is much more slowly decomposed by water. Beckurts and Otto[2]. If both acids be boiled with water, then in

4 hours	11%	CH_2Cl	COOH	is decomposed,	and in	5 hours	but	1.3%	$CHCl_2$	COOH
11	"	23%	"	"	"	"	10	"	"	1.83%	"
37	"	56%	"	"	"	"	40	"	"	7.06%	"
96	"	82%	"	"	"	"	100	"	"	12.4%	"

(c) *Trichloracetic acid.* The aqueous solution rapidly decomposes upon boiling into carbonic acid and chloroform. Beckurts and Otto.[3] It is evident then that the dichloracetic acid is the most stable.

The chlorine derivatives of toluol exhibit a similar characteristic.

[1] Berichte der deutschen chemischen Gesellschaft, 4, 341.
[2] Berichte der deutschen chemischen Gesellschaft, 14, 583.
[3] Berichte der deutschen chemischen Gesellschaft, 14, 589.

(a) *Benzylchloride.* Limpricht[1] says, "Sodium decomposes benzylchloride when heated." "When it is boiled with water for twenty-three hours, it is almost completely decomposed." Niederist.[2] Zincke[3] states that finely divided copper acts vigorously upon benzylchloride when heated, forming cuprous chloride and evolving hydrochloric acid.

(b) *Benzalchloride.* "Sodium and potassium can be boiled together with 'chlorobenzol' (heated to 206°) without the metal losing its luster or the slightest trace of the chloride being formed." Wicke.[4]

"When 'chlorobenzol' is boiled together with sodium for a long time, it is decomposed with the formation of a brown resin." Beilstein.[5]

"Boiling 'chlorobenzol' is in reality, although very slowly, somewhat decomposed by sodium." Limpricht.[6]

Cahours[7] could obtain no trace of benzaldehyde upon heating "benzolchloride" with water in sealed tubes at 130°–140°. Limpricht[8] obtained benzaldehyde in "varying quantities" under the same conditions, except a higher temperature, 160°, was employed. Lippmann and Hawliczek[9] were unable to obtain it until a temperature of 250° was attained. Onufrowicz[10] obtained stilbene chloride by treatment with copper at 100.°

(c) *Benzotrichloride.* Limpricht[11] obtained benzoic acid from this compound by the action of water in sealed tubes at 140°–190°. Hanhart[12] remarks, "Benzotrichloride is energetically acted upon by copper when gently warmed." When boiled upon the water bath with sodium for eight hours, a perceptible quantity of sodium chloride is formed. Here, as in the previous case, the disubstituted body is least acted upon.

[1] Liebig's Annalen, 139, 306.
[2] Liebig's Annalen, 196, 353.
[3] Berichte der deutschen chemischen Gesellschaft, 2, 739; 4, 298.
[4] Liebig's Annalen, 102, 363.
[5] Liebig's Annalen, 116, 349.
[6] Liebig's Annalen, 139, 317.
[7] Liebig's Annalen, Supplementband 2, 306.
[8] Liebig's Annalen, 139, 319.
[9] Berichte der deutschen chemischen Gesellschaft, 9, 1462.
[10] Berichte der deutschen chemischen Gesellschaft, 17, 835.
[11] Liebig's Annalen, 139, 324.
[12] Berichte der deutschen chemischen Gesellschaft, 12, 681.

PREPARATION OF ORTHODICHLORSTILBENE CHLORIDE.

Fifteen grams of orthochlorbenzalchloride were dissolved in 70 grams methyl alcohol, and heated upon an oil bath for six hours to 95°, with a mixture of 25 grams (3 mols.) molecular silver and 25 grams ignited sea sand. The hot alcoholic solution was separated from the silver and the alcohol evaporated in a current of dry air until a sirup began to separate out. The crystals which were obtained at the same time were pressed between filter paper and crystallized after the following method: petroleum ether boiling from 33° to 55° was added to the ethereal solution, until a cloud was formed; it was then filtered and evaporated in a current of dry air. By this means the body was obtained as white rhombic crystals of melting point 170.5°, and analyzed as follows:

I. 0.2415 gram yielded 0.0682 gram H_2O and 0.4613 gram CO_2,
0.2378 gram gave 0.4239 gram AgCl.

II. 0.1859 gram yielded 0.0537 gram H_2O and 0.3546 gram CO_2,
0.2149 gram gave 0.3841 gram AgCl.

Calculated for $C_{14}H_{10}Cl_4$.		Found.	
		I.	II.
C	52.50	52.10	52.01
H	3.12	3.06	3.21
Cl	44.37	44.06	44.20

The compound is easily soluble in ether and chloroform, difficultly in methyl alcohol and absolute ethyl alcohol and naphtha. The yield is about 5% – 8% of the orthochlorbenzalchloride employed.

PREPARATION OF ORTHODICHLORSTILBENE.

Twenty-five grams orthochlorbenzalchloride are poured upon a mixture of 33 grams (4 mols.) of reduced copper and 66 grams ignited sea sand, which is contained in the tube in which the reaction is to take place. The object of the sand is to absorb the chloride completely, and to separate the particles of copper so that they do not pack together, and thus diminish the surface. The tube is now sealed and heated for 4 – 5 hours in an oil bath to 105°. Upon opening the

tube there is usually no pressure and no hydrochloric acid escapes; the tube is cut into small pieces and its contents extracted with dry acetone. The acetone solution is dried with ignited sulphate of sodium, evaporated in distilling flasks of about 50 cc. capacity and distilled at a pressure of 100 mm.

If the whole be distilled at one time, much is lost in the tar which is always formed. The main portion goes over between 160° and 260°; of this, the larger quantity boils at about 220° and crystallizes upon cooling; very little boils lower, but a considerable quantity of a resinous body still containing chlorine remains in the flask. The crystals, as obtained from absolute alcohol, are long white needles, and melt at 97°; from toluol they are rhombic crystals. The figures obtained by analysis agree with the formula $C_{14}H_{10}Cl_2$.

I. 0.1975 gram yielded 0.0741 gram H_2O and 0.4853 gram CO_2,
0.1387 gram gave 0.1601 gram AgCl.

II. 0.2371 gram yielded 0.0876 gram H_2O and 0.5813 gram CO_2,
0.2578 gram gave 0.2961 gram AgCl.

Calculated for $C_{14}H_{10}Cl_2$.		Found. I.	Found. II.
C	67.47	67.14	66.90
H	4.01	4.17	4.00
Cl	28.51	28.53	28.38

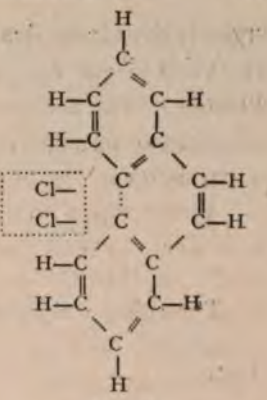
Orthodichlorstilbene is readily soluble in alcohol, ether, benzol, toluol, chloroform and carbon bisulphide, difficultly in naphtha (bpt. 110°); the yield is 10% – 12% of the chloride used.

The compound can have either the formula

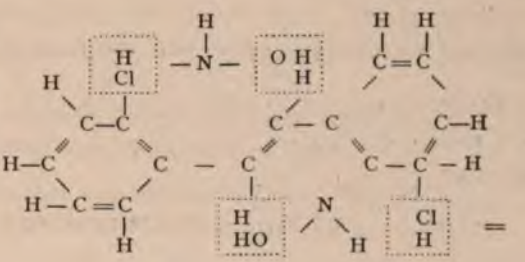
$$\begin{array}{l} C_6H_4Cl-C-H \\ \qquad\quad\;\, \| \\ C_6H_4Cl-C-H \end{array} \quad \text{or} \quad \begin{array}{l} C_6H_4Cl-C-H \\ \qquad\quad\;\, \| \\ \qquad H-C-C_6H_4Cl. \end{array}$$

If it be plane-symmetric according to the first formula, then by elimination of the chlorine it should give phenanthrene.

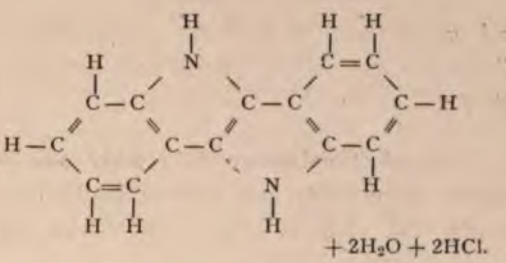
To this end it was treated in alcoholic solution with zinc dust, and also in a sealed tube at 190° with metallic sodium, neither of which had the slightest action upon it. By the action of cement copper in a combustion tube at a red heat, a small quantity of chloride of copper was formed, and also a small quantity of an oil that still contained chlorine. Similar results were obtained under like conditions with zinc dust.



If, however, the compound possessed the axial-symmetric formula, then when heated with hydroxylamine it should give a double pyrrol derivative, $C_{14}H_{10}N_2$.



Upon heating in a sealed tube with hydroxylamine for five or six hours, to 140°, no reaction was perceptible. Aniline and alcoholic ammonia under the same conditions were without action.



$+ 2H_2O + 2HCl.$

All attempts to change the compound into an isomer by the usual method of heating to 190° were fruitless. If the body in boiling chloroform solution be treated with chlorine, there results after cooling, a white crystalline compound which is identical with the orthodichlorstilbene dichloride previously described. It melts at 170.5°, and upon analysis, 0.2468 gram substance gave 0.4449 gram AgCl or 44.47%; the theory requiring 44.37% chlorine.

PREPARATION OF ORTHOCHLORTOLANE HYDROCHLORIDE.

The dichloride, when treated with one molecule of sodium hydrate in alcoholic solution in a sealed tube at 100°, gave a white substance

crystallizing in scales of a melting point of 66° and easily soluble in alcohol and ether. It corresponds with the formula $C_{14}H_9Cl_3$ as the analysis shows.

I. 0.2370 gram substance gave 0.3560 gram AgCl.
II. 0.2103 " " " 0.3212 " " (according to lime method).

Calculated for $C_{14}H_9Cl_3$.	Found.	
Cl 37.49	I. 37.14	II. 37.76

The reaction by which the body was formed is as follows: —

$$C_6H_4<\begin{matrix}Cl\\CHCl-ClCH\end{matrix}>C_6H_4+NaOH=C_6H_4<\begin{matrix}Cl\\CH=ClC\end{matrix}>C_6H_4+NaCl+H_2O.$$

PREPARATION OF ORTHODICHLORTOLANE.

If in the preceding reaction an excess of sodium hydrate be used, then two molecules of hydrochloric acid are split off according to the equation

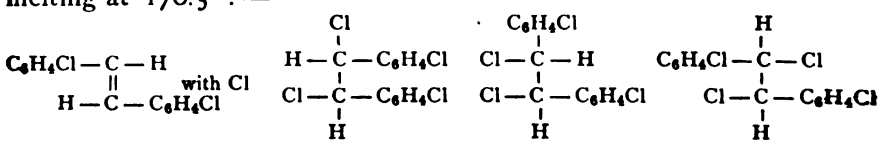
$$C_6H_4<\begin{matrix}Cl\\CHCl-ClHC\end{matrix}>C_6H_4+2NaOH=C_6H_4<\begin{matrix}Cl\\C=C\equiv\\C\end{matrix}>C_6H_4+NaCl+2H_2O,$$

and white rhombic tables of melting point 88° – 89° are the result. These are the orthodichlortolane, and are easily soluble in alcohol and ether. The analysis: —

I. 0.2824 gram yielded 0.0905 gram H_2O and 0.7019 gram CO_2.
I. 0.2246 " " 0.2604 " AgCl.
II. 0.2242 " " 0.2571 " "

Calculated for $C_{14}H_8Cl_2$.	Found.	
	I.	II.
C 68.01	67.79	
H 3.24	3.56	
Cl 28.75	28.67	28.43

By the treatment of the axial-symmetric body with chlorine, it is possible to form the three following compounds, of which the first would probably be the most easily formed, and very likely is the one melting at 170.5°: —



If this compound be treated with one molecule of potassium hydrate, there would be formed from all three the plane symmetric orthodichlortolane hydrochloride $\begin{matrix} C_6H_4Cl-C-H \\ \| \\ C_6H_4Cl-C-Cl \end{matrix}$, with an excess of potassium hydrate, orthodichlortolane $\begin{matrix} C_6H_4Cl-C \\ \vdots \\ C_6H_4Cl-C \end{matrix}$. This compound would be formed the most readily from the first orthodichlorstilbene dichloride.

From the plane-symmetric orthodichlorstilbene there could be also three orthodichlorstilbene dichlorides of the following constitution: —

$$\begin{matrix} H-C-C_6H_4Cl \\ \| \\ H-C-C_6H_4Cl \end{matrix} \quad \text{with Cl} \quad \begin{matrix} H \\ | \\ Cl-C-C_6H_4Cl \\ | \\ Cl-C-C_6H_4Cl \\ | \\ H \end{matrix} \qquad \begin{matrix} C_6H_4Cl \\ | \\ Cl-C-Cl \\ | \\ Cl-C-C_6H_4Cl \\ | \\ H \end{matrix} \qquad \begin{matrix} Cl \\ | \\ C_6H_4Cl-C-H \\ | \\ Cl-C-C_6H_4Cl \\ | \\ H \end{matrix}$$

If these be treated with one molecule of potassium hydrate, there would be obtained from all three the axial-symmetric orthodichlortolane hydrochloride $\begin{matrix} Cl-C-C_6H_4Cl \\ \| \\ C_6H_4Cl-C-Cl \end{matrix}$ from which the orthodichlortolane would be obtained with much greater difficulty than from the plane-symmetric.

As but one of the six possible isomers could be obtained by the addition of chlorine to orthodichlorstilbene, it is impossible to draw any very definite conclusions. From the ease with which hydrochloric acid is split off by potassium hydrate, one might conclude that the one obtained came from the axial-symmetric orthodichlorstilbene, and has probably the formula $\begin{matrix} Cl \\ | \\ H-C-C_6H_4Cl \\ | \\ Cl-C-C_4H_6Cl \\ | \\ H \end{matrix}$

TRIPLE-EXPANSION EXPERIMENTAL ENGINE

In the Engineering Laboratories of the Massachusetts Institute of Technology.

By CECIL H. PEABODY, S.B.

One of the most important pieces of apparatus found in the engineering laboratories in the building recently erected on Trinity Place is the triple-expansion experimental engine, built by the E. P. Allis Company, of Milwaukee, Wisconsin. The engine is of the Corliss type, and has three cranks arranged as shown by Figures 1 and 2. The diameter of the high-pressure cylinder is 9 inches; of the intermediate cylinder, 16 inches; and of the low-pressure cylinder, 24 inches; the stroke of each is 30 inches. Each cylinder is steam-jacketed on the heads and barrel, and steam may be applied directly to any or all of the cylinders; so that by proper combinations of piping and valves the engine may be run simple, compound, or triple. The intermediate receivers are also provided with steam jackets, so that they may act as reheaters. The exhaust steam is received and condensed by a surface condenser of the marine type, which drains into an independent direct-acting air pump. The condensed water from the air pump is collected and weighed in tanks on scales, covered to avoid surface evaporation. The several jackets on the cylinders and intermediate receivers, five in all, are drained into separate receivers made of 5-ft. sections of 8-in. pipe, closed at the ends and provided with glass water-tubes and scales, so that the condensed water may be collected and measured. Provision is made for applying the indicators, gauges, and thermometers required for making a complete thermodynamic test of the engine. The cylinders, receivers, and piping are thoroughly wrapped with asbestos paper, hair felt, and lagging, or canvas.

The engine is belted to the main line of shafting running through the middle of the laboratory, and provides power for lighting the

Fig. 1.

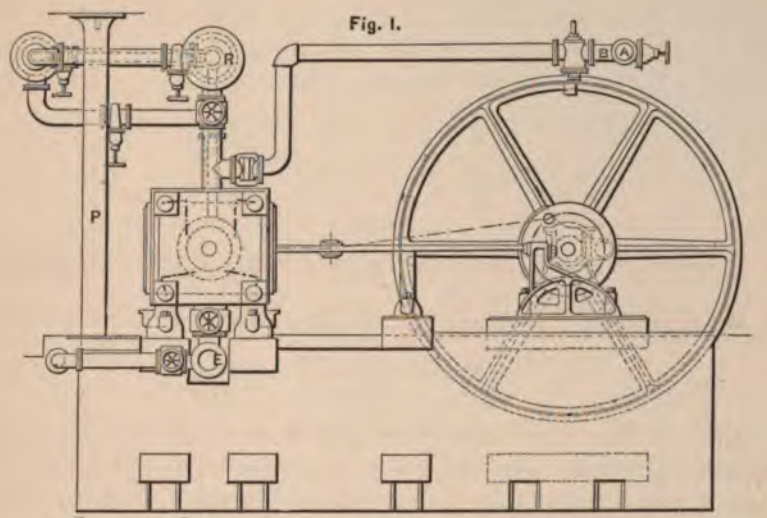
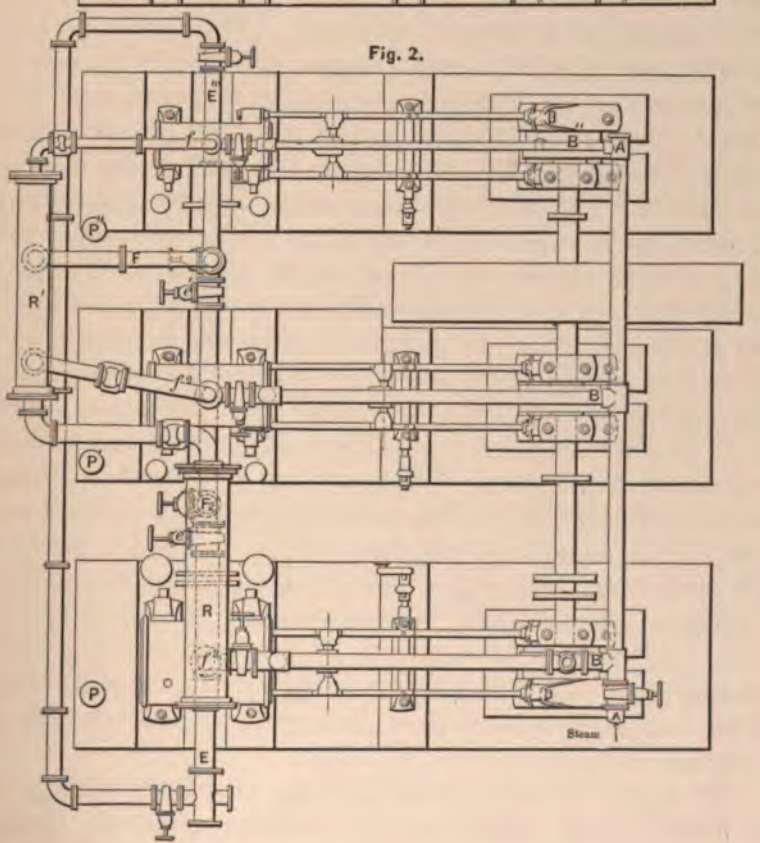


Fig. 2.



entire building by electricity, and for various tests requiring power. There are now in the laboratory, the dynamo, which absorbs about 50 horse-power; two water-cooled friction brakes that can absorb 50 horse-power each; rotary and centrifugal pumps, and other apparatus requiring power. The short section of shafting which receives the driving-belt can be uncoupled from the line-shafting running each way; considering the friction of this short section with the friction of the engine, the friction of the several lines of shafting and of the brakes when unloaded, and also the load on the dynamo, can be determined by the aid of dynamometers, and thus the load on the engine may be determined, and also the friction of the engine.

Figures 1 and 2 give an elevation and plan intended to show the general arrangement of the foundations, frames, and the piping. Parts receive the same letters in both figures. The section of the city on which the building is located being made land, the foundation was made by piling over the entire area on which the engine stands, and capping the piles with several courses of large granite blocks. From the granite the engine beds are carried up through a sub-basement so as to place the engine conveniently with reference to the engine room or basement floor. Each cylinder stands on blocks of cut stone, and is bolted through stones built into the foundation. The engine shaft is carried by splayed cast-iron bearings which are supported and bolted to the foundation in a similar manner. Each cylinder is connected to the shaft-bearings in front of it by wrought-iron rods, to take up the thrust and pull of the piston and connecting-rod. These rods are supported at about mid-length, and, from the support to the cylinder, are square, and act as guides to the crosshead. The fly wheel is placed between the high and intermediate engines, the location being decided by the fact that it is the common custom to disconnect the low-pressure cylinder, and run the high-pressure and intermediate cylinders compound and non-condensing. The cranks for the high and low pressure engines are overhung; the intermediate crank is a solid cast-steel crank bolted to flanges on the ends of the two sections of the shaft.

A 6-in. pipe A runs through the laboratory and supplies steam at about 150 lbs. pressure for heating, for various experiments, and for running the engine. At B, B′, and B″ are branches running to the several cylinders, each with a stop valve near the cylinder for the convenience of the engineer. Near B is a reducing valve or regulator,

for reducing the pressure of steam to 40 lbs. to the square inch or less, when the low-pressure cylinder receives steam direct. R and R′ are the first and second intermediate receivers. R′ is set over beyond the columns P′, P″, to make room for the driving-belt. An exhaust pipe E runs under all the cylinders, and into this each cylinder may be directly exhausted. Stop valves e and e' divide the exhaust pipe into sections when required. A valve under the low-pressure cylinder shuts it off from the exhaust pipe when the large engine is disconnected.

The high-pressure cylinder may communicate with the intermediate receiver R′ by the pipe f for admission, and by the pipe F for exhaust. The intermediate cylinder communicates with the receiver R′ by the pipe f_2 for admission, and with the receiver R by the pipe F_2 for exhaust. The large cylinder receives steam from the receiver R through the pipe f_3. Finally the two receivers are connected by a pipe with a stop valve as shown. There is a roundabout exhaust with valves at each end for the high-pressure cylinder. Normally the high-pressure cylinder receives steam from the pipe B″ and exhausts by the pipe F to the receiver R′; the intermediate cylinder receives steam from the receiver R′ by the pipe f_2 and exhausts to the receiver R by the pipe F_2, and the low-pressure cylinder receives steam from the receiver R by the pipe f_3 and exhausts through the valve beneath it into the pipe E.

Among the combinations rendered possible, the following may be noticed. Steam may be supplied through the reducing valve at B to the low-pressure cylinder and through the receiver R to the receiver R′, and so to the intermediate cylinder; meanwhile the high-pressure cylinder receives full-pressure steam as usual, and exhausts by the roundabout pipe; all the exhaust steam may be used for heating at about 4 lbs. back pressure. When a test is to be made, the engine is readily changed to triple-expansion condensing.

The condenser used for experimental work only, is set in a sub-basement, so that the exhaust pipe drains into it, and it in turn drains into a direct-acting air pump, from which the condensed water is received in weighing-tanks.

Steam for jacketing the high-pressure cylinder and the first intermediate receiver R′ is taken from the direct supply of steam to the high-pressure cylinder by a ¾-in. pipe; this pipe has two branches, one to the receiver and the other to the cylinder. The ¾-in. branch pipe leading to the cylinder is again branched into three parts, one to

the barrel and one to each of the heads; each of the three jackets has a pet-cock to discharge the air when the jackets are supplied with steam, and to reveal leakage of steam when the valves are shut. Thus the barrel or the heads, or both, may be jacketed with steam.

The intermediate cylinder and second receiver R are supplied with jacket steam through a ¾-in. pipe from the steam pipe B′ leading to the intermediate cylinder; a reducing valve allows the pressure on the jackets to be made lower than boiler pressure when desired. The low-pressure cylinder receives jacket steam from the pipe B before the reducing-valve, and the pipe for this purpose has its own reducing-valve to control the pressure in the jackets. The jackets are designed to carry full boiler pressure on all the cylinders when desired.

The drainage from the three jackets (*i.e.*, the barrels and two heads) of each of the cylinders is collected and led directly under the pressure in the jackets to three measuring-vessels in the sub-basement. The drainage from the receiver jackets is similarly disposed of. When the receiver jackets are not in use the receivers themselves are drained into the measuring-vessels, and an arrangement of two valves with pet-cock between makes it sure that the drainage comes from the jackets or from the receivers, as the case may be, and reveals leakage should it occur.

With the large number of large valves it is apparent that some method of detecting leakage must be devised if the tests on the engine are to receive credence. The valves are of a good type, are new, and have been carefully handled to avoid injury. Some of them may be tested for leakage while the engine is running, others can be shut and tested for leakage before the engine is started. To arrange for starting the engine a one-inch by-pass is carried round the stop valves of the intermediate and low-pressure cylinders. The by-pass pipe has two valves, and on the space between is a pet-cock that can be used to test for leakage of the valve on the by-pass pipe, or for leakage past the stop valve into the steam chest.

The three cylinders of the engine have valve gears of the Corliss type; those of the high and intermediate cylinders are arranged as usual and are moved by an eccentric acting on a wrist-plate. Vacuum dashpots insure a rapid and quiet closing of the admission valves. Since the low-pressure cylinder has a volume somewhat more than twice that of the intermediate cylinder, a special valve gear with two eccentrics is arranged as shown by Figures 3 and 4. The wrist-plate P

is moved by an eccentric with a little angular advance, suitable for giving release and compression, and is connected to the exhaust valves at V and V_1 in the usual manner. The wrist-plate P_1, outside of P, receives motion from an eccentric with a large negative, angular advance, and the admission valves are given a large lead when in mid-position in compensation. Thus the release mechanism may be arranged to give a cut-off longer than half-stroke, as the proportions of the cylinder require.

The governor is attached to the frame of the intermediate cylinder, and, by the aid of a horizontal shaft running in front of the three cylinders, may control the releasing mechanism of each of the cylinders. In Figure 4, *a* is the horizontal shaft controlled by the governor. A vertical link acts on the arm *b* of a bell-crank lever above the cylinder, and from another arm *c* a link is carried to the collar *d* on the crank and valve spindle and from thence to a collar *e* on the head and valve spindle. When desired, the governor may be disconnected by dropping the link from *a* to *b*, and the disengagement gear may then be set by hand, while the engine is running, by aid of the handwheel and screw at A; of course the hand adjustment is thrown out of gear when the governor has control of the releasing-gear. At S is a starting-bar for moving the admission valves by hand, when the gab-rod is disconnected for the admission wrist-plate.

Fig. 5.

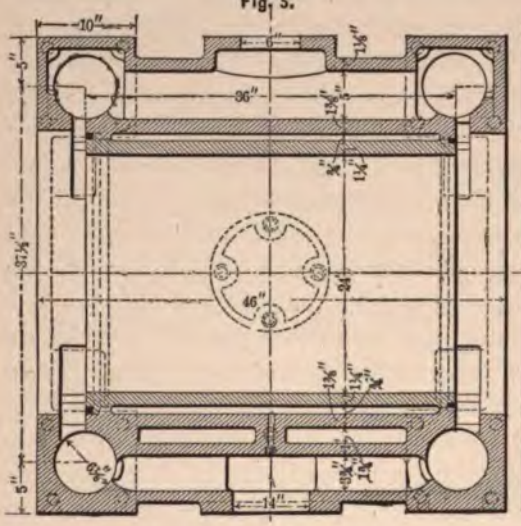
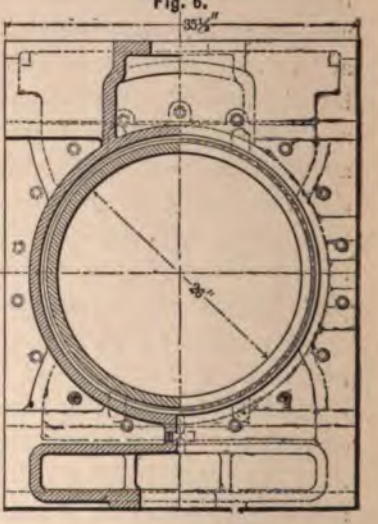


Fig. 6.



Figures 5 and 6 give transverse and longitudinal sections of the large or low-pressure cylinder. The barrel jacket is made by bush-

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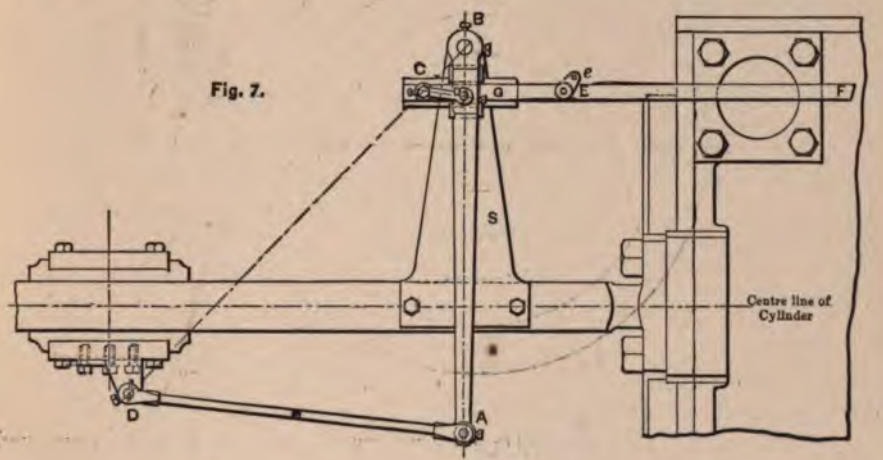
ing the cylinder-casting, as shown, the bushing being forced in under hydraulic pressure, and made tight by a ring of copper driven into a dovetail groove. The ends of the cylinders are jacketed by coring out the cylinder heads.

Fig. 7.



The indicator motion for the intermediate cylinder is shown by Figure 7. S is a standard bolted to the engine frame near the crank end cylinder head. A B is a swinging lever, connected by the link A D to the engine crosshead, and by the parallel link C G to the rod E F. The rod E F slides in guides, one on the standard S as shown, and the other on the head end valve bonnet at the right hand, and it is at the proper height for the indicator cord. The crank end indicator cord is hooked directly on to the pin *e*, which is on an arm E *e*, that can be swung up or down to give a small vertical adjustment, and clamped by a thumbscrew. The other indicator has a similar arrangement. All the joints of the indicator motion have split brass bushings and set screws to take up lost motion. When worn out the bushings can be renewed. Thus at A the link is forked, and embraces the end of the lever A B; the pin is fast in the fork, and turns in a brass bushing in the end of the lever, with a set screw near A.

Fig. 8.

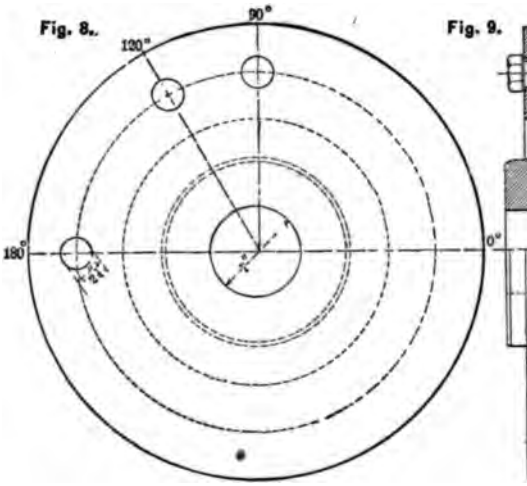
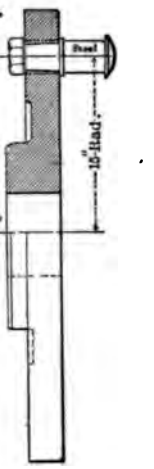


Fig. 9.



The angles at which the several cranks follow can be arranged by a device shown by Figures 8 and 9. The disc has three distinct holes bored in it, making angles of 90°, 120°, and 180°. The steel crank-pin is made tapering, and fits into a tapering steel bushing that is cut in two at one side, and part way through at seven other places. A heavy hexagonal nut on the back of the disc draws the crank-pin in till its collar bears on the face of the disc, and at the same time forces the taper bushing in, and secures the pin in the disc. This device has been found to work well on this engine, which develops about 150 horse-power, or 50 horse-power on each crank when running triple with 150 lbs. steam pressure and at 80 revolutions per minute.

APPLICATION OF HIRN'S ANALYSIS TO MULTIPLE-EXPANSION ENGINES.[1]

By CECIL H. PEABODY, S.B.

The application to simple engines of Hirn's theory, or, more properly, of Hirn's analysis of the interchange of heat between the steam and the walls of the cylinder, is now familiar to the engineering profession, through the labors of Hirn, Hallauer, Dwelshauvers-Dery, Mair, and others. But the writer has not been able to find a clear and correct statement of the analysis for compound and multiple-expansion engines. Applications of the analysis to compound engines are given by Hallauer in the form of numerical calculations only, and he contents himself with finding the heat rejected from the walls of the cylinder during exhaust. Among the very important tests on large engines reported by Mair[2] are several on compound engines; but he has united in one term the algebraic sum of the quantity of heat absorbed by the low-pressure cylinder walls during admission, and that yielded during expansion a quantity which in his tests is sometimes positive and sometimes negative, and which does not appear to have any physical meaning.

The statement of the analysis given here is a development of those given by Dwelshauvers-Dery[3] and Mair, making use of the latter's method of representing the heat equivalent of the intrinsic energy of all the fluid in the cylinder by one character. An application is given to experiments made on the triple-expansion engine in the engineering laboratories of the Massachusetts Institute of Technology.

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[1] Presented at the Providence Meeting (1891) of the American Society of Mechanical Engineers.

[2] Minutes of Proceedings of the Institution of Civil Engineers, **70**, 313; **79**, 323.

[3] Revue Universelle des Mines, **8**, 362, 1880; also Trans. Am. Soc. Mech. Engs. **11**, 72.

SIMPLE ENGINES.

Suppose that M pounds of steam are admitted to the cylinder of a simple engine per stroke, having in the supply pipe the pressure p pounds per square inch absolute, and the quality x; *i.e.*, each pound is x part steam mingled with $1-x$ part water. The heat brought into the cylinder per stroke, reckoned from freezing point, is

$$Q = M(q + x r) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

in which q is the heat of the liquid and r is the latent heat of vaporization. Should the steam be superheated in the supply pipe to the temperature t_s, then

$$Q = M\,[q + r + c_p\,(t_s - t)] \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

in which t is the temperature of saturated steam at the pressure p, and $c_p = 0.4805$ is the specific heat of superheated steam at constant pressure.

Let the heat equivalent of the intrinsic energy of all the water and steam in the cylinder at any instant be represented by I. Let M_0 be the weight of steam caught in the cylinder at compression. Then we shall have:

at admission	$I_0 = M_0\,(q_0 + x_0\,\rho_0)$	(3)
at cut-off	$I_1 = (M + M_0)\,(q_1 + x_1\,\rho_1)$	(4)
at release	$I_2 = (M + M_0)\,(q_2 + x_2\,\rho_2)$	(5)
at compression	$I_3 = M_0\,(q_3 + x_3\,\rho_3)$	(6)

in which, q_0, q_1, q_2, q_3, and ρ_0, ρ_1, ρ_2, and ρ_3 are respectively the heats of the liquid, and the internal latent heats, or heat equivalents of the internal work, at the absolute pressures p_0, p_1, p_2, and p_3, measured on the indicator diagram, at admission, cut-off, release, and compression.

If the work under any line of the indicator diagram, reckoned from a perfect vacuum, is represented by W, and if A is the reciprocal of the mechanical equivalent of heat (equal to $\frac{1}{778}$), then the heat equivalents of the external work done by or on the piston are:

during admission	$A\ W_a$
during expansion	$A\ W_b$
during exhaust	$A\ W_c$
during expansion	$A\ W_d$

Usually heat is absorbed by the walls of the cylinder during admission of steam and up to the point of cut-off, heat is yielded

during expansion and during exhaust, while the interchange during compression is uncertain. It is possible that the walls may receive heat during expansion, and it is conceivable that they should give heat to the steam during the entire cycle; consequently it is convenient to affect all of the interchanges of heat by the same sign and to assume that heat absorbed by the wall is positive; in the solution of examples a negative sign will then mean that heat is yielded by the walls.

The interchanges of heat may be expressed as follows:

during admission	$Q_a = Q + I_0 - I_1 - A\ W_a$	(7)
during expansion	$Q_b = I_1 - I_2 - A\ W_b$	(8)
during exhaust	$Q_c = I_2 - I_3 - Mq_4 - G\,(q_k - q_i) + A\ W_c$ . .	(9)
during compression	$Q_d = I_3 - I_0 + A\ W_d$	(10)

In equation (9) the expression Mq_4 represents the heat carried away by the condensed steam flowing away from a surface condenser at the temperature t_4, and $G\,(q_k - q_i)$ represents the heat carried away by G pounds of cooling water, which enters with the temperature t_i and leaves with the temperature t_k.

Let the volume of one pound of water in cubic feet be σ, and let the increase of volume due to vaporization be u, so that the volume of one pound of dry steam is $u + \sigma$, while the volume of one pound of moist steam is $xu + \sigma$. Let the volume of the clearance of the engine be V_0 cubic feet, and let the volume developed by the piston at cut-off and release be V_1 and V_2, while the volume remaining at compression is $V_3 + V_0$. The volumes of the mixed fluid in the cylinder are:

at admission	$V_0 = M_0\,(x_0\,u_0 + \sigma)$	(11)
at cut-off	$V_0 + V_1 = (M + M_0)\,(x_1\,u_1 + \sigma)$	(12)
at release	$V_0 + V_2 = (M + M_0)\,(x_2\,u_2 + \sigma)$	(13)
at compression	$V_0 + V_3 = M_0\,(x_3\,u_3 + \sigma)$	(14)

An examination of the three groups of equations, (3) to (6), (7) to (10), and (11) to (14), will show that all that is requisite for an entire solution of our problem is the knowledge of the quality of the steam at one of the four events of the cycle. It is customary to assume the steam at compression to be dry and saturated, or that x_3 is unity. Such an assumption gives for the weight of steam weight at compression,

$$M_0 = \frac{V_0 + V_3}{u_3 + \sigma} = (V_0 + V_3)\,\gamma_3 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

in which γ_3 is the density or weight of one cubic foot of saturated steam at the absolute pressure at the beginning of compression.

Applying this result to equations (11) to (13) gives:

$$x_0 = \frac{V_0}{M_0 u_0} - \frac{\sigma}{u_0} \quad \ldots \quad (16)$$

$$x_1 = \frac{V_0 + V_1}{(M + M_0) u_1} - \frac{\sigma}{u} \quad \ldots \quad (17)$$

$$x_2 = \frac{V_0 + V_2}{(M + M_0) u_2} - \frac{\sigma}{u_2} \quad \ldots \quad (18)$$

Having the quality of the steam at admission, cut-off, and release, the several values of I at these points and at compression may be found from equations (3) to (6), and finally the interchanges of heat may be calculated by aid of equations (7) to (10). It may happen that the assumption of dry steam at compression may make the steam in the cylinder at admission appear to be superheated, *i.e.*, x_0 will appear to be larger than unity; in such case it will be convenient to assume the steam to be saturated at admission also. The superheating is commonly small, and the error from the assumption of dry steam at compression and at admission is believed to be insignificant. In some of the tests to be quoted later the steam in the intermediate and low-pressure cylinders is superheated at release by the action of jackets supplied with steam at the boiler pressure. In such case the intrinsic energy of one pound of superheated steam may be calculated by aid of the equation [1]

$$E = 3pv + 778 \times 857.2 \quad \ldots \quad (19)$$

in which v is the specific volume in cubic feet, and p is the pressure in pounds on the square foot. Which will give at release

$$I_2 = (V_0 + V_2) \frac{3 \times 144\, p_2}{778} + 857.2\,(M + M_0) \quad \ldots \quad (20)$$

in which p_2 is the absolute pressure per square inch at release.

Let the heat lost by radiation, condensation, etc., be Q_r, and let Q_j be the heat supplied by the steam jacket when one is used. If the jacket is supplied by a separate pipe, or if in any way the water condensed therein can be withdrawn and measured, the value of Q_j is readily determined for any test; Q_e may be determined by collecting the condensation in the jackets while the engine is at rest.

[1] Thermo-dynamics of the Steam Engine, p. 123, by the author.

Of the heat supplied to the cylinder per stroke, a part is changed into work, a part is carried away by the condensed steam and the cooling water, and the remainder is lost by radiation ; therefore

$$Q_e = Q + Q_f - Mq_4 - G\,(q_b - q_i) - A\,W \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

in which W is the work represented by the area of the indicator diagram, or

$$W = W_a + W_b - W_c - W_d \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

Equation (21) may be used to calculate Q_e when it cannot be determined directly ; but such a calculation is vitiated by all the errors of determining the several quantities in that equation. When Q_e is determined directly, equation (21) may be considered a check on the accuracy of the determination of all the quantities appearing in it ; it is not a check on the determination of the interchanges by equations (7) to (10).

If a test is made on a non-condensing engine, or if for any reason the terms depending on the condenser cannot be determined, then these terms may be eliminated from equation (9) by aid of equation (21), giving

$$Q_e = I_2 - I_3 - Q - Q_f + Q_e + A\,(W + W_c) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

It may be of interest to note that this equation is equivalent to Hirn's direct determination of the exhaust waste, and that equation (9) is equivalent to his second determination or check. It is hardly necessary to call attention to the fact that such a check appears much more striking in an arithmetical calculation than in the algebraic statement of the analysis. Exception must be taken to the idea that the exhaust waste can be considered as a test of the efficiency of an engine. While it is true that a large exhaust waste causes a poor economy, the extinction of the exhaust waste, by lavish use of steam jackets or otherwise, may be dearly bought. This is well shown by Mair's tests.

COMPOUND ENGINES.

The equations (7), (8), and (10), with equation (23), may be applied directly to the high-pressure cylinder of a compound engine, giving :

$$Q_a = Q + I_0 - I_1 - A\,W_a \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

$$Q_b = I_1 - I_2 - A\,W \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

$$Q = I_2 - I_3 - Q - Q_f + Q_e + A\,(W + W_c) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

$$Q_d = I_3 - I_0 + A\,W_d \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

By aid of equation (21) the term Q may be eliminated from the equation for the interchange of heat during admission; the other equations, (8) to (10), can be used unchanged in form. The interchanges of heat in the low-pressure cylinder can then be found by the equations:

$$Q'_a = Q'_e - Q'_j + Mq_4 + G(q_4 - q_e) + I_0 - I_1 + A(W' - W'_a) \quad (28)$$

$$Q'_b = I'_1 - I'_2 - A W'_b \quad (29)$$

$$Q'_c = I'_2 - I'_3 - Mq - G(q_4 - q_i) + A W'_c \quad (30)$$

$$Q'_d = I'_3 - I'_0 + A W'_d \quad (31)$$

These equations may be applied to any compound engine, whether it has a receiver or not, and without taking account of the loss or gain of heat experienced by the steam passing from the high to the low pressure cylinder. Thus it may be applied when there is a reheater between the cylinders, even though the heat thus imparted is not determined. It is apparent that this method cannot be extended to a triple-expansion engine.

MULTIPLE-EXPANSION ENGINES.

For multiple-expansion engines, the following method may be used. The heat rejected by the high-pressure cylinder during exhaust is

$$Q + Q_j - A W - Q_e \quad (32)$$

This heat passes into the first intermediate cylinder, and from there, with the gain or loss experienced, proceeds to the next cylinder. The sum, or difference, may be taken for Q'_1 the heat brought into the next cylinder per stroke. The same operation may be applied to each successive cylinder, and the final result may be checked by aid of the data depending on the condenser.

Compound and multiple-expansion engines, having various arrangements of steam-jackets and reheaters, will require special modifications of the form of the analysis. If two or more steam-jackets have a common drain, or if steam is passed in succession through several jackets, then the distribution of the heat among the several jackets must be determined or estimated.

The equations for the interchanges of heat in the triple engine of the Massachusetts Institute of Technology are as follows, the terms

Q_{eR} and Q_{jR} representing the heat radiated from the surface of an intermediate receiver and the heat supplied by condensation of steam in the jacket of such a receiver:

High-pressure cylinder,

$$Q = M(xr + q) \quad (33)$$
$$Q_a = Q + I_0 - I_1 - A\,W_a \quad (34)$$
$$Q_b = I_1 - I_2 - A\,W_b \quad (35)$$
$$Q_c = I_2 - I_3 - Q - Q_j + Q_e + A\,(W + W_c) \quad (36)$$
$$Q_d = I_3 - I_0 + A\,W_d \quad (37)$$

Intermediate cylinder,

$$Q' = Q + Q_j - A\,W - Q_e + Q_{jR} - Q_{eR} \quad (38)$$
$$Q'_a = Q' + I'_0 - I'_1 - A\,W'_a \quad (39)$$
$$Q'_b = I'_1 - I'_2 - A\,W'_b \quad (40)$$
$$Q'_c = I'_2 - I'_3 - Q' - Q'_j + Q'_e + A\,(W' + W'_c) \quad (41)$$
$$Q'_d = I'_3 - I'_0 + A\,W'_d \quad (42)$$

Low-pressure cylinder,

$$Q'' = Q' + Q'_j - A\,W' - Q'_e + Q'_{jR} - Q'_{eR} \quad (43)$$
$$Q''_a = Q'' + I''_0 - I''_1 - A\,W''_a \quad (44)$$
$$Q''_b = I''_1 - I''_2 - A\,W''_b \quad (45)$$
$$Q''_c = I''_2 - I''_3 - Q'' - Q''_j + Q''_e + A\,(W'' + W''_c) \quad (46)$$

or
$$Q''_c = I''_2 - I''_3 - M_{q4} - G\,(q_k - q_i) + A\,W''_d \quad (47)$$
$$Q''_d = I''_b - I''_0 + A\,W''_d \quad (48)$$

Finally,

$$\Sigma\,Q_e = Q_e + Q_{eR} + Q'_e + Q'_{eR} + Q''_e = Q + Q_j + Q_{jR} + Q'_j + Q'_{jR} + Q''_j - M_{q4} - G\,(q_k - q_i) - A\,(W + W' + W'') \quad (49)$$

APPLICATION OF ANALYSIS TO INSTITUTE OF TECHNOLOGY ENGINE.

The experimental engine in the engineering laboratories of the Massachusetts Institute of Technology is a three-crank horizontal triple-expansion engine with two intermediate receivers, built by E. P. Allis & Co., of Milwaukee. The receivers and the cylinders are thoroughly steam-jacketed by separate pipes from the main steam-pipe,

and drain individually into five receptacles for gathering and measuring the condensation. The jackets on the heads and barrels of the cylinders can be applied separately or together. Reducing valves are arranged to supply the jackets of the second receiver and the intermediate and low-pressure cylinder with steam at less than boiler pressure. The cranks are usually set 120° apart, the high-pressure crank leading, but the angle between the cranks can be varied. Steam is supplied to the engine through a 6-inch pipe carried under ground about 1,000 feet from the Rogers Building. Condensation is withdrawn from the pipe when it enters the building, and for further precaution the steam is passed through a Stratton separator on the way to the engine. The quality of the steam is determined near the throttle-valve by a throttling calorimeter. The exhaust from the engine is led to a surface condenser, beneath which is a Blake direct-acting air pump which discharges the condensed steam in weighing tanks. The automatic governor is arranged to control the cut-off of any or all of the cylinders; or, when the automatic gear is disconnected, the cut-off may be varied by hand. During all of the tests the governor controlled the cut-off of the high-pressure cylinder, and the cut-off on the other cylinders was adjusted to give expansion down to the back-pressure line in the high-pressure and intermediate cylinders. The low-pressure cylinder has two wrist-plates, so that the cut-off may be made longer than half stroke. The piping of the engine is arranged so that it may be run in several combinations; before making a test or series of tests all valves are arranged and tested to guard against leakage.

The main dimensions of the engine are as follows:

Diameter of the high-pressure cylinder	9	inches.
Diameter of the intermediate cylinder	16	"
Diameter of the low-pressure cylinder	24	"
Diameter of the piston-rods	$2\frac{1}{16}$	"
Stroke	30	"

Clearance in per cent. of the piston displacements:

High-pressure	cylinder,	head end,	8.83;	crank end,	9.76
Intermediate	"	"	10.4	"	10.9
Low-pressure	"	"	11.25	"	8.84

The following table gives the data and results of some tests made for the application of Hirn's analysis, by the class graduating this year:

	I.	II.	III.	IV.
Duration of test, minutes	60	60	60	60
Total number of revolutions	5299	5228	5173	5148
Revolutions per minute	88.3	87.1	86.2	85.8
Steam consumption during test, pounds:				
Passing through cylinders	1193	1157	1234	1305
Condensation in h. p. jacket	57	50	29	30
" in first receiver jacket	61	64	69	72
" in inter. jacket	85	92	97	105
" in second receiver jacket	53	50	52	51
" in l. p. jacket	89	76	90	87
Total	1538	1489	1571	1650
Condensing water for test, pounds	22847	22186	20244	20252
Priming, by calorimeter	0.013	0.012	0.011	0.012
Temperatures, Fahrenheit:				
Condensed steam	95.4	92.1	102.4	105.3
Condensing water, cold	41.9	42.1	43.0	42.8
Condensing water, hot	96.1	96.6	106.3	109.6
Pressure of the atmosphere, by the barometer, pounds per square inch	14.8	14.8	14.7	14.7
Boiler pressure, pounds per square inch, absolute	155.3	155.5	156.9	157.7
Vacuum in condenser, inches of mercury	25.0	25.1	24.1	23.9
Events of the stroke:				
High-pressure cylinder —				
Cut-off, crank end	0.192	0.194	0.245	0.283
head end	0.215	0.205	0.271	0.305
Release, both ends	1.00	1.00	1.00	1.00
Compression, crank end	0.05	0.05	0.04	0.04
head end	0.05	0.05	0.05	0.05
Intermediate cylinder —				
Cut-off, both ends	0.29	0.29	0.29	0.29
Release, both ends	1.00	1.00	1.00	1.00
Compression, crank end	0.03	0.03	0.03	0.03
head end	0.04	0.04	0.04	0.04
Low-pressure cylinder —				
Cut-off, crank end	0.38	0.38	0.38	0.38
head end	0.39	0.39	0.39	0.39
Release, both ends	1.00	1.00	1.00	1.00

	I.	II.	III.	IV.
Absolute pressures in the cylinder, pounds per square inch:				
High-pressure cylinder —				
Cut-off, crank end	145.9	145.9	138.8	138.3
head end	143.2	143.1	140.3	140.6
Release, crank end	41.3	41.5	44.7	48.4
head end	41.5	40.5	45.7	49.8
Compression, crank end	43.7	45.3	48.5	53.2
head end	48.7	47.9	54.5	62.0
Admission, crank end	64.5	68.8	72.2	81.2
head end	75.3	74.8	86.7	97.8
Intermediate cylinder —				
Cut-off, crank end	37.2	37.6	38.6	40.9
head end	35.0	35.3	39.6	42.6
Release, crank end	13.6	14.2	14.7	16.0
head end	13.4	13.8	14.9	16.0
Compression, crank end	16.3	17.3	18.2	19.0
head end	17.9	18.8	20.3	22.4
Admission, crank end	20.4	20.8	22.2	23.1
head end	21.1	22.8	24.2	26.7
Low-pressure cylinder —				
Cut-off, crank end	12.1	12.6	12.4	13.2
head end	12.0	12.4	13.1	14.0
Release, crank end	5.6	5.3	5.1	5.7
head end	5.4	5.8	5.9	6.4
Compression and admission —				
crank end	3.7	3.8	4.1	4.2
head end	4.3	4.5	4.6	4.7
Heat equivalents of external work, B. T. U. from areas on indicator diagram to line of absolute vacuum:				
High-pressure cylinder —				
During admission, $A\ W_a$, crank end	5.71	5.78	7.00	8.19
head end	6.61	6.37	8.42	9.50
During expansion, $A\ W_b$, crank end	10.65	10.76	10.40	10.25
head end	10.81	11.04	11.22	11.09
During exhaust, $A\ W_c$, crank end	7.73	7.89	8.44	9.02
head end	8.08	8.15	9.04	9.66
During compres'n, $A\ W_d$, crank end	0.48	0.60	0.49	0.50
head end	0.62	0.64	0.73	0.81
Intermediate cylinder —				
During admission, $A\ W_a$, crank end	7.58	7.57	7.98	8.64
head end	7.43	7.55	8.46	9.10
During expansion, $A\ W_b$, crank end	9.54	9.54	9.91	10.64
head end	9.22	9.31	10.37	11.14

	I.	II.	III.	IV.
Intermediate cylinder —				
During exhaust, $A\ W_c$, crank end	9.27	9.47	9.64	10.54
head end	9.27	9.47	10.18	10.84
During compres'n, $A\ W_d$, crank end	0.39	0.43	0.57	0.46
head end	0.60	0.70	0.78	0.84
Low-pressure cylinder —				
During admission, $A\ W_a$, crank end	7.75	7.95	8.33	8.97
head end	7.99	8.19	8.66	9.39
During expansion, $A\ W_b$, crank end	6.83	7.10	6.86	7.45
head end	6.87	7.12	7.34	7.87
During exhaust, $A\ W_c$, crank end	5.08	5.08	4.62	5.09
head end	5.08	5.16	4.81	5.00
During compres'n, $A\ W_d$, crank end	0.00	0.00	0.00	0.00
head end	0.00	0.00	0.00	0.00
Quality of the steam in the cylinder. At admission and at compression the steam was assumed to be dry and saturated:				
High-pressure cylinder —				
At cut-off . . . . . x_1	0.785	0.784	0.848	0.875
At release . . . . . x_2	0.899	0.903	0.920	0.931
Intermediate cylinder —				
At cut-off . . . . . x_1	0.899	0.912	0.906	0.908
At release . . . . . x_2	0.994	super-heated.	super-heated.	super-heated.
Low-pressure cylinder —				
At cut-off . . . . . x_1	0.978	super-heated.	0.970	0.974
At release . . . . . x_2	super-heated.	super-heated.	super-heated.	super-heated.
Interchanges of heat between the steam and the walls of the cylinders, in B. T. U. Quantities affected by the positive sign are absorbed by the cylinder walls; quantities affected by the negative sign are yielded by the walls:				
High-pressure cylinder —				
Brought in by steam . . . Q	132.93	130.77	141.11	149.84
During admission . . . Q_a	23.54	23.43	17.49	14.93
During expansion . . . Q_b	−18.69	−19.28	−15.33	−14.03
During exhaust . . . Q_c	−8.36	−7.22	−3.50	−2.38
During compression . . . Q_d	0.45	0.51	0.49	0.52
Supplied by jacket . . . Q_j	4.56	4.08	2.39	2.50
Lost by radiation . . . . Q_e	1.50	1.52	1.54	1.54
First intermediate receiver —				
Supplied by jacket . . . Q_{jR}	4.92	5.20	5.67	5.95
Lost by radiation . . . . Q_{eR}	0.58	0.58	0.59	0.59

	I.	II.	III.	IV.
Intermediate cylinder —				
Brought in by steam . . . Q' . .	131.89	129.61	137.87	146.64
During admission . . . . Q'_a . .	13.62	11.74	11.33	11.75
During expansion . . . Q'_b . .	—18.65	—18.84	—20.30	—21.88
During exhaust . . . . Q'_c . .	0.22	1.57	2.88	3.41
During compression . . . Q'_d . .	0.44	0.51	0.62	0.59
Supplied by jacket . . . Q'_j . .	6.82	7.50	7.97	8.64
Lost by radiation . . . . Q'_e . .	2.45	2.48	2.50	2.51
Second intermediate receiver —				
Supplied by jacket . . . Q'_{jR} .	4.20	4.04	4.27	4.22
Lost by radiation . . . Q'_{eR} .	1.20	1.22	1.23	1.24
Low-pressure cylinder —				
Brought in by steam . . Q'' . .	132.14	130.50	138.61	147.33
During admission . . . Q''_a . .	5.85	3.05	5.57	5.29
During expansion . . . Q''_b . .	—9.51	—7.09	—8.65	—10.13
During exhaust . . . . Q''_c . .	2.53	2.23	—1.44	—0.11
During compression . . . Q''_d . .	0.00	0.00	0.00	0.00
Supplied by jacket . . . Q''_j . .	7.08	6.20	7.41	7.14
Lost by radiation . . . Q''_e . .	4.34	4.40	4.45	4.47
Total loss by radiation:				
By preliminary test . . ΣQ_e . .	10.07	10.20	10.31	10.35
By equation (49)	11.68	10.19	8.75	8.07
Power and economy:				
Heat equivalents of works per stroke:				
High-pressure cylinder . AW . .	8.44	8.34	9.17	9.52
Intermediate cylinder . . AW' .	7.12	6.95	7.77	8.42
Low-pressure cylinder . AW'' .	9.64	10.06	10.87	11.79
Totals	25.20	25.35	27.81	29.73
Total heat furnished by jackets . . .	27.58	27.02	27.71	28.45
Distribution of work:				
High-pressure cylinder	1.00	1.00	1.00	1.00
Intermediate cylinder	0.84	0.83	0.85	0.88
Low-pressure cylinder	1.14	1.21	1.19	1.24
Horse-power	104.9	104.2	113.1	120.3
Steam per horse-power per hour . . .	14.65	14.31	13.90	13.73
B. T. U. per horse-power per minute .	247	241	236	232

An investigation of the results in the table shows that the difference in economy depends on the cut-off of the high-pressure cylinder, which is at about $\frac{1}{6}$ stroke for the first two, and at about $\frac{1}{4}$ stroke for the last two. The quantities that are most notably affected by the change of cut-off are the heat furnished by the high-pressure jacket and the condition of the steam, and interchanges of heat in the high-pressure cylinder. Thus the heat supplied by the high-pressure jacket for the tests, with the cut-off at $\frac{1}{4}$ stroke, is about half that furnished in the tests at $\frac{1}{6}$ stroke; which finds explanation or confirmation in the facts, that the steam is much dryer at cut-off with the longer cut-off, and that both the heat absorbed and yielded by the walls of the cylinder is notably less with that cut-off. The general accuracy and correctness of the tests is shown by the comparison of the total radiation, as found by collecting and weighing the jacket condensation when the engine was at rest, and by calculating the same by aid of the terms depending on the condenser. As has already been pointed out, this does not serve as a check on the interchanges of heat, but the regularity of the results for the intermediate and low-pressure cylinders must be considered to be very satisfactory, especially when it is remembered that any error made in work on the high-pressure and intermediate cylinders is carried on through all the remainder of the test. It may not be out of place to call attention to the fact that the four tests were made by four divisions of the class, and to say that carelessness or inaccuracy of a single observer might readily derange all the results of the test.

It is notable that the steam becomes dryer in its course through the engine, under the influence of the thorough steam-jacketing, with steam at boiler pressure, and that it is practically dry at release, in both the intermediate and low-pressure cylinders. In some of the tests it appears to be superheated at this point, and in one, even at cut-off in the low-pressure cylinder. The superheating is in no case large, and as it is accompanied by a small positive value for Q_e, the exhaust-waste, it may be that the steam was really dry and saturated at those points, though superheating by jackets filled with steam at so high a pressure is not impossible. Since a positive value for Q_e indicates that the walls absorbed heat during exhaust, such a result, if large, would be absurd; but in these tests it may properly be looked upon as the error, being in no case larger than about two per cent. of

the value of Q; if the steam at release is either dry or superheated, the value of Q must be very small or zero.

It is most remarkable that the heat furnished by the jackets is nearly as large in amount as that changed into work in the tests with the cut-off at $\frac{1}{4}$ stroke, and is actually larger for the tests with the cut-off at $\frac{1}{6}$ stroke. Also, that the heat lost by radiation is one third as much as the heat changed into work, if not more; the engine being arranged for convenient experimental work, probably has a large radiation, though it is thoroughly wrapped and lagged on all hot surfaces.

ON THE DETERMINATION OF THE ELECTROLYTIC DISSOCIATION OF SALTS BY MEANS OF SOLUBILITY EXPERIMENTS.

By ARTHUR A. NOYES, Ph.D.

Contents. — 1. General theory of solubility effect. 2. Special case where the two salts have one ion in common. 3. Solubility experiments a method of determining dissociation. Purpose of this investigation. 4. Description of the experiments. 5. Experiments with thallous chloride and the alkali chlorides. 6. Dissociation of the alkali chlorides. 7. Experiments with thallous chloride and hydrochloric acid. 8. Dissociation of hydrochloric acid. 9. Experiments with thallous chloride and thallous nitrate and chlorate. Dissociation of the last two salts. 10. Experiments with thallous chloride and magnesium, calcium, barium, copper, zinc, and manganese chlorides. 11. Dissociation of these bivalent-metal chlorides. 12. Experiments with thallous chloride and cadmium chloride; dissociation of the latter. 13. Experiments with thallous chloride and mercuric and lead chlorides. 14. Experiments with lead chloride and potassium, magnesium, calcium, manganese, zinc, and cadmium chlorides, and hydrochloric acid. 15. Theory of solubility effect between two ternary electrolytes. 16. Experiments with lead chloride and mercuric chloride and lead nitrate. 17. Conclusion.

I. GENERAL THEORY OF SOLUBILITY EFFECT.

The effect which one salt exerts on the solubility of another has been the subject of numerous and extended investigations. These yielded, however, nothing more than a mass of empirical results, until two years ago, when it was shown by Nernst[1] that the electrolytic dissociation theory offered an explanation of this phenomenon of solutions, as it had already done of so many others. The bearing of this theory on the matter of solubility will be evident from the following considerations:

A saturated solution is to be regarded as a condition of equilibrium between the undissolved substance on the one hand, and the portion of it dissolved in the solvent on the other. It is, moreover, a condition of equilibrium independent of the amount of the solid body in contact

[1] Zeitschrift für physikalische Chemie, 4, 372.

with the solution — a fact ordinarily expressed by saying that the active mass of the undissolved substance is constant. The active mass of the dissolved portion, which holds the equilibrium with this, must likewise be constant as long as the temperature, the solvent, and the chemical nature of the dissolved substance remain unchanged. Although this statement seems self-evident, yet, before the establishment of the electrolytic dissociation theory, it could not be consistently maintained. For it was frequently observed that the solubility of a salt was greatly influenced by the addition of another, even in cases where a chemical reaction between them seemed impossible, and where the small quantity of the second substance added made the assumption of an essential change in the nature of the solvent very improbable; and there was at that time no reason to regard the active mass in the solution as anything else than the total quantity of dissolved substance. The electrolytic dissociation theory, however, has shown that salts, acids, and bases, in aqueous solution, undergo a partial, often nearly complete, dissociation into their electrolytic components or ions. A dilute solution of sodium chloride, or of nitric acid, for example, contains comparatively few molecules of the composition NaCl or HNO_3, but these have been broken up by the act of solution into $\overset{+}{Na}$ and $\overset{-}{Cl}$, or $\overset{+}{H}$ and $\overset{-}{NO_3}$, ions, which exist as molecules chemically independent of each other. Returning now to the case of a saturated solution, it is clear in the light of this theory that the total quantity of dissolved substance has entirely lost its chemical significance as an active mass: that which in the solution does hold the equilibrium with the undissolved substance is either the ions or the undissociated portion of the salt, or most probably both together. Which of these three possibilities is actually the case does not concern us; for, according to the general law of dissociation, the active mass of the undissociated substance in the solution must be proportional to the product of the active masses of its dissociation products, in this case the ions. If, now, as in a saturated solution, the system is so conditioned that one of these quantities is constant, it follows that the other must be so likewise. The following two principles may therefore be laid down as the basis of the theory of solubility effect:

1. In a saturated solution of a partly dissociated substance, the undissociated portion remains unchanged in quantity, even though a second dissociated substance be added.

2. The product of the active masses of the dissociation products (the ions) of the substance with which the solution is saturated remains constant also.

The validity of these principles depends on two conditions: first, that the addition of the second substance be not so great nor of such a nature as to cause a sensible change of the solvent; and second, that no special chemical action between the dissolved substances, like, for example, the formation of a double salt, comes into play.

2. SPECIAL CASE WHERE THE TWO SALTS HAVE ONE ION IN COMMON.

Let us consider the case where one of the ions of the substance added is identical with one of those of the body with which the solution is saturated; for example, suppose hydrochloric acid added to a saturated solution of potassium chloride. By this addition a new quantity of chlorine ions is brought into the solution; therefore, in order that the product of the quantities of potassium and chlorine ions may remain constant, the quantity of the potassium ions must decrease; in other words, potassium chloride is precipitated — its solubility is diminished. This case is completely analogous with dissociation in the gaseous condition; for example, the introduction into a space saturated with ammonium chloride vapor of one of its decomposition products, ammonia or hydrochloric acid gas, causes, as is well known, a condensation of a portion of the salt.

As the mathematical expression for the influence on the solubility in the case of binary electrolytes, Nernst gives the following formula:

$$m_0^2 a_0^2 = ma\,(ma + xa_1);$$

in which m_0 denotes the solubility of the body in pure water; m, its solubility after the addition of the quantity x of another substance with one of the same ions; a_0, the dissociation of the first substance in saturated solution before the addition; a, its dissociation after the addition of the second substance; and a_1 the dissociation of the latter. This formula is the expression of the second principle given above; it states the constancy of the product of the quantities of the two ions; for $m_0 a_0$ is plainly the quantity of each of them before the addition of the second salt; ma is the quantity of the ion not added, and $(ma + xa_1)$, that of the added ion, after the addition.

As expression of the first principle of solubility effect, which has reference to the undissociated portion, we have the following simple formula:

$$m_0 (1 - a_0) = m (1 - a).$$

Solving these two equations for m, we get:

$$m = -\frac{xa_1}{2a} + \sqrt{m_0^2 \frac{a_0^2}{a^2} + \frac{x^2 a_1^2}{4a^2}}$$

$$m = m_0 \frac{1 - a_0}{1 - a}$$

It is evident that by means of these formulæ, from the solubility of a salt in pure water, its solubility in the presence of any added amount x of another salt can be calculated, provided the degrees of dissociation (a_0, a_1, and a) of the two salts are known. The dissociation of a salt, however, may be determined, at least approximately, by taking the ratio of the molecular electrical conductivity of its solution at the concentration in question, to its molecular conductivity at extreme dilution,[1] where it is completely dissociated. It was the special object of an investigation which I made last year,[2] to test Nernst's theory of solubility above described; and it was, in fact, proved by experiments with eleven different pairs of salts that the results calculated by means of this theory are in quite close agreement with those experimentally obtained. Differences, however, did occur, which were undoubtedly far greater than the experimental errors; they lay, moreover, always in the same direction. There is good reason to believe that the cause of these deviations is the incorrectness of the values of the dissociation used in the calculation; that is to say, that it lies in the inaccuracy of the assumption that dissociation and molecular conductivity are exactly proportional, and not in the invalidity of the laws of chemical mass action, which are the basis of the theory of solubility effect.

[1] Arrhenius, Zeitschrift für physikalische Chemie, 1, 632.

[2] Zeitschrift für physikalische Chemie, 6, 241.

To give an idea of the magnitude of these differences, I tabulate below the results of two series of experiments described in my previous article :

$AgBrO_3$ with $AgNO_3$ and $KBrO_3$ (24.5°).

Added Quantity.	Solubility in the presence of $AgNO_3$	Solubility in the presence of $KBrO_3$	Calculated Solubility.
0	0.00810	0.00810	. . .
0.00850	0.00510	0.00519	0.00504
0.0346	0.00216	0.00227	0.00206

$TlCl$ with $TlNO_3$ and HCl (25°).

Added Quantity.	Solubility in the presence of $TlNO_3$.	Solubility in the presence of HCl.	Calculated Solubility.
0	0.0161	0.0161	. . .
0.0283	0.00830	0.00836	0.00783
0.0560	0.00571	0.00565	0.00508
0.1468	0.00332	0.00316	0.00253

3. SOLUBILITY EXPERIMENTS A METHOD OF DETERMINING DISSOCIATION. PURPOSE OF THIS INVESTIGATION.

This discrepancy between the observed and calculated values of the solubility suggested the application of solubility experiments as an independent method of determining the dissociation of salts ; for, just as the solubility m of one salt in the presence of another can be calculated, if the values of the dissociation are known, so conversely can the degree of dissociation be determined from the values of the solubility experimentally obtained. Of the various physical properties of solutions which are dependent on the dissociation of the dissolved substance, the electrical conductivity is the only one which, up to the present time, has furnished any accurate measure of it. Other properties, like the freezing point and the vapor pressure, differ, in the case of dilute solutions, so little from those of the pure solvent, that the experimental errors make them properties of little value in the determination of the dissociation ; while for concentrated solutions they give inaccurate results, owing to the deviation from the law of Avo-

gadro in their case. From the electrical conductivity of their solutions, the dissociation of a very large number of organic acids has been determined by Ostwald and others, and the values so obtained varied with the concentration in almost all cases in the manner required by the general laws of dissociation; which fact alone is very strong evidence of their accuracy. In the case of the better conductors, however, like most salts, strong acids and bases, the dissociation indicated by the conductivity does not change in accordance with the laws of dissociation; but it decreases with the increase of concentration much more slowly than those laws require, which is itself an indication of incorrectness. Especially after I had found that the solubility effect indicated a smaller dissociation in the more concentrated solutions than that deduced from the conductivity, did there exist strong reasons for doubting the exact proportionality between dissociation and conductivity, and for giving greater confidence to the results of solubility experiments.

These considerations, and the great importance of an accurate knowledge of the dissociation constants in all applications of the theory of electrolytic dissociation, led me to undertake the present investigation, which has for its special object the study of the dissociation of the chlorides of the various metals.

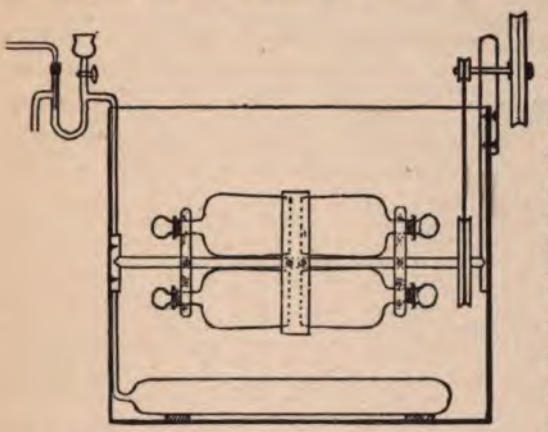
4. DESCRIPTION OF THE EXPERIMENTS.

Two series of experiments were made, in one of which thallous chloride, and in the other of which lead chloride was taken as the substance with which the solution was saturated; and the solubility of each of these two compounds in solutions containing different known quantities of the various metallic chlorides was determined.

A 0.2 normal and a 0.05 normal solution of each of the chlorides whose effect on the solubility was to be determined, were prepared by weighing out the solid substances when this was practicable, and by titration of the solutions with silver nitrate when it was not. Exactly 100 cc. of each of these solutions were placed in a small bottle, and an excess of the thallous or lead chloride was added in the form of a fine, dry powder. The bottles were closed with tightly fitting cork stoppers, and placed in a bath kept at a constant temperature by means of a regulator of the kind described by Ostwald.[1] All experi-

[1] Zeitschrift für physikalische Chemie, 2, 564.

ments were made at a temperature of 25°. In order to secure continuous agitation, and thereby rapid saturation, a machine was constructed in the manner illustrated in the accompanying sketch.



A horizontal brass shaft, set in bearings within the thermostat, had soldered to it at its middle point two annular metal bands of such diameter that the bottoms of the bottles to be rotated just fitted into them. In order to keep the bottles firmly in their horizontal position, the neck of each was clasped between two elastic pieces of metal attached to the shaft near its ends. Four bottles were in this way provided for, and, by means of rubber bands passing around them, four more could be fastened to the shaft, so that eight were easily rotated at one time. A rubber belt passing over a wheel near the end of the shaft, and over a second smaller wheel on a horizontal axle at the top of the thermostat, served to transfer the power from a small electric motor. The shaft made about twenty revolutions per minute; and the agitation of the solutions was sufficient to keep the finer particles of the solid substances in suspension, while the coarser particles rolled along the sides of the bottle as it turned over and over. The solutions were rotated for three hours; the bottles were then detached from the shaft and placed in an upright position on a perforated shelf within the bath, where the solutions were allowed to settle for three to four hours. By means of a pipette 50 cc. of the clear solution were then taken out and submitted to analysis; 50 cc. of distilled water were then added to the 50 cc. remaining in the bottle, and the solution so obtained, containing the added substance in only half the previous concentration (0.1 or 0.025 normal), was ready for a second rotation. In the case of thallous chloride, experiments were made with four different dilutions: 0.2, 0.1, 0.05, and 0.025 normal; with lead chloride the last was omitted, owing to its much greater solubility.

5. EXPERIMENTS WITH THALLOUS CHLORIDE AND THE ALKALI CHLORIDES.

The substances whose effect on the solubility of thallous chloride was first tried were the chlorides of the alkalies, potassium, sodium, and ammonium. The potassium and sodium chlorides used were prepared from the commercial chemically pure salts by precipitation of the solutions with hydrochloric acid and subsequent ignition of the precipitate; the ammonium chloride was purified by precipitation and subsequent sublimation.

As the experiments in contemplation involved the making of more than a hundred thallium determinations, it was highly desirable, in fact almost indispensable, to find a simple volumetric method. Of the various processes which have been proposed, the titration with potassium permanganate, by which the thallous is converted to a thallic salt (Tl_2O to Tl_2O_3), was found to be by far the most satisfactory. As this method, which was discovered by E. Willm,[1] seems to have been little used, and as it gives thoroughly reliable results, it will perhaps be well to describe it briefly.

A solution of permanganate, approximately $\frac{1}{25}$ normal, was standardized against thallous sulphate, which is easily obtained perfectly pure by crystallization, and which is anhydrous and not hygroscopic. About 0.1 of a gram of this is dissolved in water, the solution is diluted to 200 cc., 10 cc. of hydrochloric acid of 1.2 specific gravity are added, and the permanganate solution is added rather slowly till the slightest pink color is observable. The end point is exceedingly sharp, and the color persists for several minutes. The thallium solution must be diluted to the extent indicated, in order to avoid precipitation of thallous chloride. Within certain limits the amount of hydrochloric acid added is a matter of indifference; for example, it was found that with 12 cc. the end point was as sharp as with 10 cc. With 20 cc., however, the pink color of the end point disappeared quite rapidly, so that the results were unreliable. With 8 cc., the brown hydrated manganese dioxide separated towards the end of the titration. The presence of metallic chlorides was found by special experiments to have no influence on the result. This method of analysis was used in all cases where chlorides were added to the thallous chloride.

[1] Répertoire de chimie pure, 5, 352, (1863).

The results obtained with the three alkali chlorides are given in the following tables. The numbers indicate here, as throughout this article, the number of equivalents dissolved in a litre.

TABLE I. TlCl in the presence of KCl, NaCl, and NH_4Cl.

Salt added.	Its Concentration.	Solubility of TlCl.	Mean of Experiments.	Mean for the three Salts.
None.	0	0.01611[1]	. . .	. . .
. . .	. . .	0.01619[1]	. . .	. . .
. . .	. . .	0.01619[2]	0.01612	0.01612
. . .	. . .	0.01604[2]	. . .	. . .
. . .	. . .	0.01607[2]	. . .	. . .
KCl.	0.025	0.00871	0.00872	. . .
. . .	. . .	0.00872	. . .	. . .
NaCl.	. . .	0.00870	0.00869	0.00872
. . .	. . .	0.00868	. . .	. . .
NH_4Cl.	. . .	0.00876	0.00877	. . .
. . .	. . .	0.00878	. . .	. . .
KCl.	0.05	0.00591	. . .	. . .
. . .	. . .	0.00594	0.00593	. . .
. . .	. . .	0.00593	. . .	. . .
NaCl.	. . .	0.00593	0.00592	0.00592
. . .	. . .	0.00590	. . .	. . .
NH_4Cl.	. . .	0.00594	. . .	. . .
. . .	. . .	0.00589	0.00593	. . .
. . .	. . .	0.00597	. . .	. . .
KCl.	0.1	0.00400	0.00399	. . .
. . .	. . .	0.00398	. . .	0.00397
NaCl.	. . .	0.00395	0.00395	. . .
. . .	. . .	0.00394	. . .	. . .
KCl.	0.2	0.00267	0.00265	. . .
. . .	. . .	0.00263	. . .	. . .
NaCl.	. . .	0.00273	0.00271	0.00269
. . .	. . .	0.00268	. . .	. . .
NH_4Cl.	. . .	0.00269	0.00271	. . .
. . .	. . .	0.00273	. . .	. . .
KCl.	0.8	0.00170	0.00170	. . .

[1] Chlorine determined by Volhard's method. [2] Thallium determined by titration with potassium permanganate.

From these results it is evident that the three substances, potassium, sodium, and ammonium chlorides, have practically the same effect on the solubility of thallous chloride, and that this holds true for all the concentrations studied. It follows, therefore, at once, that these three chlorides are equally dissociated at the same concentrations; or, in other words, that they have the same dissociation constant.

6. DISSOCIATION OF THE ALKALI CHLORIDES.

Before, however, we can determine the absolute values of the dissociation of the added salt, it is necessary that we know something about that of the thallous chloride. For the diminution of the solubility of a salt depends on its own dissociation as well as on that of the added salt. Now, unfortunately, we have no accurate knowledge of the dissociation of thallous chloride; nevertheless, it is probable that it is dissociated to the same extent as the alkali chlorides, since the three univalent metal chlorides investigated agree so completely in this respect; and it seems especially so when the great similarity of the potassium and thallium salts is considered. It deserves mention, moreover, in this connection, that potassium, sodium, and thallous hydrates have been shown by Ostwald to be equally dissociated, for they saponify acetic ether at the same rate.[1]

At any rate, this very probable assumption has to be made preliminarily, and it is the basis of the calculations made in this article. If at any time more accurate knowledge is obtained of the dissociation of thallous chloride, the results can be corrected correspondingly. It will now be seen that in the calculation of the dissociation of the added salt this assumption suffices, no more specific knowledge of the dissociation of the thallous chloride being required.

The two equations deduced at the beginning of this article as the mathematical expression of the fundamental principles of solubility effect are:

$$m_0 (1 - a_0) = m (1 - a) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and

$$m_0^2 a_0^2 = ma (ma + xa_1) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

These equations contain three unknown quantities, a_0, a, and a_1, and cannot, therefore, of themselves be solved. If, however, we make

[1] Journal für praktische Chemie, 35, 121. The constants representing the relative velocities of the reactions in the three cases are 161, 162, and 158, respectively. The solutions employed were rather dilute, however; viz., $\frac{1}{40}$ normal.

the assumption just discussed, that the dissociation constant C has the same value for the added and for the original salt, we have according to the laws of dissociation for the former,

$$C\,x\,(1-a_1) = xa_1\,(ma + xa_1),$$

and for the latter,

$$C\,m\,(1-a) = ma\,(ma + xa_1);$$

whence

$$\frac{a_1}{1-a_1} = \frac{a}{1-a}, \text{ or } a_1 = a.$$

That is, the dissociation of the added salt in the saturated solution of the original salt is the same as the dissociation of the latter in the solution of the former. Equation (2), therefore, becomes

$$m_0^2 a_0^2 = m^2 a_1^2 + mxa_1^2,$$

and combining this with equation (1) with elimination of a_0 and solving for a_1, we get

$$a_1 = \frac{m_0 - m}{x}\left(1 + \sqrt{1 + \frac{x}{m}}\right) \quad \ldots\ldots\ldots \quad (3)$$

by means of which the dissociation corresponding to the various concentrations can be calculated.

It is, however, to be observed that the dissociation a_1 is not the dissociation of the added quantity x in *pure water*, but is its dissociation in the presence of the quantity m of the salt with which the solution is saturated. In order to determine the concentration, y, of the salt in pure water to which the dissociation a_1 corresponds, consider the two dissociation formulæ,

$$C = \frac{xa_1\,(ma + xa_1)}{x\,(1-a_1)} \text{ and } C = \frac{y^2 a_1^2}{y\,(1-a_1)},$$

whence follows on combination,

$$y = x + m\frac{a}{a_1} \quad \ldots\ldots\ldots\ldots\ldots \quad (4)$$

or, in the case under present consideration, where $a = a_1$, $y = x + m$.

In Table II are given the values of the dissociation calculated from the results of the foregoing table by formula (3). As the variations in the solubility effect of the three different salts are small and irregular, and probably due mainly to experimental error, only a single cal-

culation for each concentration has been made; and in this calculation the mean values of m given in the fifth column of Table I have been used. In the first column of the following table is given the value of x, the added quantity of the salt; in the second, that of $y = m + x$, the concentration of the salt in pure water, to which the dissociation a_1, given in per cent. in the third column, corresponds. In the fourth column, under C, is given the value of the dissociation constant $C = \frac{1 - a_1}{a_1^2 y}$. In the fifth, under 100 a_1 (calc.), is given the value of a_1 corresponding to the mean value of the constant 4.4. And in the sixth column is given a_0, the dissociation of the thallous chloride in saturated solution in pure water (concentration = 0.01612), calculated by means of formula (1) from each value of m and a_1.

TABLE II. Dissociation of KCl, NaCl, and NH_4Cl.

x.	y.	$100a_1$.	C.	$100a_1$ (calc.)	$100a_0$.
0.025	0.0337	87.8	4.7	88.4	93.4
0.050	0.0559	83.1	4.4	83.0	93.8
0.100	0.1040	74.3	4.5	74.6	93.7
0.200	0.2027	65.0	4.1	63.8	94.2

While the dissociation varies from 65 to 88 per cent., the value of the theoretical dissociation constant remains nearly the same. It is to be remembered that errors in the dissociation are greatly multiplied in the constant. A comparison of the figures of the fifth column, which are calculated from the mean value of the constant (4.4), with those of the third, gives a better idea than the constant itself of the degree of conformity with the law of dissociation. In the three diluter solutions the agreement is within seven tenths of a per cent., while in the most concentrated one, where the error in the dissociation, owing to the small solubility and to theoretical causes, is undoubtedly large, the variation amounts to about two per cent. The proof here given, that the laws of chemical mass action are applicable to electrolytic dissociation in the case of salts, forms one of the most important results of this investigation.

It is of especial interest to compare these values of the dissociation

and of the dissociation constant with those deduced from the electrical conductivity. In Table III the results by the two methods are placed side by side. Kohlrausch's[1] values of the conductivity of potassium chloride have been made the basis of the calculation of the dissociation, which is, as usual, assumed equal to the ratio $\left(\frac{\mu_v}{\mu\infty}\right)$ of the molecular conductivity at any dilution v to that at extreme dilution. The dissociation deduced from the solubility experiments has been interpolated from the above results for the round concentration values, 0.03, 0.05, 0.1, and 0.2, with the help of the constant.

Table III. Comparison of the Dissociation of KCl as determined from the Electrical Conductivity and from Solubility Experiments.

Concentration.	(A). Dissociation from solubility effect.	(B). Dissociation from electrical conductivity.	Constant corresponding to (A).	Constant corresponding to (B).
0.016	93.8	93.3	(4.4)	4.8
0.03	88.7	91.0	4.7	3.6
0.05	84.3	89.1	4.4	2.8
0.1	74.8	86.1	4.5	1.9
0.2	65.2	82.8[2]	4.1	1.3

It is evident from the above table that the two methods lead to practically the same dissociation only for the greatest dilution (0.016 normal), and that in all the other cases considerable differences exist, which increase in magnitude as the concentration increases, and which reach at the greatest concentration (0.2 normal) the large amount 17.6 per cent. Further, the electrical conductivity gives values of the dissociation which are uniformly greater than those deduced from the solubility experiments, the differences thus agreeing in direction with those observed in all my previous experiments.[3]

The values of the dissociation constant in the two cases need little comment. The fact that the dissociation calculated from the

[1] Annalen der Physik und Chemie, **26**, 195.
[2] Graphically interpolated from Kohlrausch's plot.
[3] Compare Zeitschrift für physikalische Chemie, **6**, 249.

solubility effect changes with the dilution in the manner required by the laws of dissociation, while that calculated from the conductivity deviates greatly from it, is of itself strong proof of the correctness of the former.

The determination of the dissociation of salts from the electrical conductivity seems therefore no longer justified except for very dilute solutions. It has always been supposed that this method was inapplicable for considerable concentrations; but Ostwald having shown its applicability up to $\frac{1}{8}$ normal in the case of many organic acids, it was assumed to be correct for equally strong solutions of salts and inorganic acids. It seems to me, however, that the results indicate that the limit of applicability of the method depends less on the concentration of the solution than on the concentration of the ions; in other words, that the electrical conductivity gives correct values of the dissociation as long as the number of ions does not exceed a certain comparatively small amount; which explains why organic acids, which are in general little dissociated, may be investigated by the method at much greater concentrations than more dissociated bodies like salts and strong acids.

The fact that the conductivity of the more concentrated solutions is greater than the dissociation of the dissolved substances justifies, is, I believe, to be explained by the participation of the water itself in the conduction; that is to say, water, which in a pure state contains only an excessively small number of ions, becomes appreciably dissociated when substances, especially electrolytes, are dissolved in it. In my previous article on solubility I did not venture to suggest this explanation, as there were no other facts to support it; but since then Le Blanc[1] has been led to the same assumption by his experiments on the minimum electromotive force required for the electrolysis of various solutions. This he found to have nearly the same value for a large number of dissimilar acids and bases, from which fact he concluded that the same substance, namely, the water, was being primarily electrolyzed in all these cases. Though the correctness of this hypothesis has not been fully proved, it must be admitted that the phenomena of electrolysis are explained much more simply and much more naturally by it than by the older, equally unproved hypothesis in which secondary actions play the principal part.

[1] Zeitschrift für physikalische Chemie, 8, 313.

7. EXPERIMENTS WITH THALLOUS CHLORIDE AND HYDROCHLORIC ACID.

The effect of hydrochloric acid on the solubility of thallous chloride was next investigated. The results are contained in the following table:

TABLE IV. TlCl in the presence of HCl.

Concentration.	Solubility of TlCl with HCl added.	Mean of Experiments.	Sol. of TlCl with KCl, NaCl, and NH_4Cl added.
0	. . .	0.01612	. . .
0.025	0.00870	0.00869	0.00872
. . .	0.00868	. . .	. . .
0.05	0.00586	0.00585	0.00592
. . .	0.00585	. . .	. . .
0.1	0.00384	0.00384	0.00397
. . .	0.00383	. . .	. . .
0.2	0.00255	0.00254	0.00269
. .	0.00254	. . .	. . .

Hydrochloric acid diminishes the solubility of thallous chloride to a somewhat greater extent than the alkali chlorides; the differences, however, are not very large. It is, therefore, dissociated to a slightly greater extent.

8. DISSOCIATION OF HYDROCHLORIC ACID.

In order to calculate its dissociation, we make use, as before, of formulas (1) and (2), which contain the three unknown quantities a_0, a_1, and a. But since a_1 and a are in this case not equal, equation (3), which results from their combination under that supposition, cannot be used. By the experiments already described, however, the value of a_0 has been determined, and is given in the last column of Table II, (mean value = 0.938). Equations (1) and (2) can now, therefore, be solved for a_1 and a, with the following result:

$$a_1 = \frac{2m_0 a_0 - (m_0 - m)}{x\left(\frac{m_0 a_0}{m_0 - m} - 1\right)} \quad \ldots\ldots\ldots\ldots \quad (5).$$

$$a = \frac{(m_0 - m)^2}{m\left(xa_1 - 2(m_0 - m)\right)} \quad \ldots\ldots\ldots\ldots \quad (6).$$

The concentration of the acid in pure water, y, to which the dissociation a_1 corresponds, is given by equation (4), as shown above.

TABLE V. Dissociation of HCl.

y	0.0337	0.0557	0.1037	0.2021
$100\alpha_1$ (for HCl)	88.2	84.7	77.7	73.5
$100\alpha_1$ (for KCl)	87.8	83.1	74.3	65.0

Concentration.	(A). Dissociation from solubility effect.	(B). Dissociation from electrical conductivity.[1]	Constant corresponding to (A).	Constant corresponding to (B).
0.03	89.1	95.7	4.6	1.6
0.05	85.4	94.6	4.0	1.2
0.1	78.1	92.1	3.6	0.9
0.2	73.2	89.8[2]	2.5	0.6

The first table contains the values calculated directly from the experiments, and the second, those interpolated for the concentrations, 0.03, 0.05, 0.1, 0.2.

Here again the dissociation deduced from the solubility effect is much less than that deduced from the electrical conductivity, the differences being even considerably greater than those in the case of the alkali chlorides. The dissociation constant corresponding to the solubility values, though much better than that corresponding to the conductivity values, is not completely satisfactory. It is evident either that the laws of dissociation are not completely followed by hydrochloric acid at the concentrations considered, or that the assumption involved in the calculation that the solvent power of the dilute hydrochloric acid is the same as that of water, is not entirely correct. It is constantly to be remembered that this assumption, which is undoubtedly invalid for very concentrated solutions, may not be exactly true in the case of certain substances in diluter solutions. At any rate, the direction in which the constant changes, since it is undoubtedly more nearly correct for the diluter solutions, indicates that the dissociation cannot be greater than the values given, but is, if anything, less. There is thus no hope of a reconciliation with the conductivity values; and those from the solubility should, it seems to me, be regarded preliminarily as the more probable.

[1] Calculated from Kohlrausch's experiments, assuming $\mu_\infty = 352$.

[2] Interpolated from Kohlrausch's plot ($\mu_v = 316.2$).

9. EXPERIMENTS WITH THALLOUS CHLORIDE AND THALLOUS NITRATE AND CHLORATE. DISSOCIATION OF LAST TWO SALTS.

The thallous nitrate used was prepared by dissolving metallic thallium in nitric acid, and crystallizing the product from hot water. The thallous chlorate was made by double decomposition between thallous sulphate and barium chlorate. The amount of thallous chloride dissolved in the presence of each salt was found, not as before by a determination of the thallium, but by one of the chlorine, made volumetrically by Volhard's method in the case of the nitrate, and gravimetrically in the case of the chlorate.

TABLE VI. TlCl in the presence of $TlNO_3$ and $TlClO_3$.

Salt added.	Its Concentration.	Solubility of TlCl.	Dissociation of added salt ($100\,\alpha_1$).	Corresponding concentration (γ).	Dissociation Constant.
None.	0	0.01612	. . .	. . .	. . .
$TlNO_3$	0.025	0.00883	85.5	0.0342	5.8
. . .	0.05	0.00626	76.5	0.0569	7.1
. . .	0.1	0.00423	67.6	0.1048	6.8
$TlClO_3$	0.025	0.00897	83.2	0.0346	7.0
. . .	0.025	0.00894	. . .	. . .	. . .

The dissociation of these two salts is in all cases a little less than that of the alkali chlorides. The constant in the case of thallous nitrate shows irregular variations, probably due to experimental errors.[1]

10. EXPERIMENTS WITH THALLOUS CHLORIDE AND MAGNESIUM, CALCIUM, BARIUM, COPPER, ZINC, AND MANGANESE CHLORIDES.

The effect of the chlorides of a number of bivalent metals on the solubility of thallous chloride was next studied. The solution of magnesium chloride was prepared by treating metallic magnesium with an insufficient quantity of hydrochloric acid, decanting the solution which had become slightly alkaline, and neutralizing it with more acid. That

[1] The results with thallous nitrate are, I fear, less accurate than the others given in this article; for, though the experiments were many times repeated, closely agreeing results were not obtained.

of calcium chloride was made by dissolving pure calcium carbonate in hydrochloric acid, adding a little lime to precipitate magnesium and other metals, filtering, and exactly neutralizing with hydrochloric acid. The commercial chemically pure salts were used in the case of barium and copper. The solution of zinc chloride was made by double metathesis between zinc sulphate and barium chloride. The manganese chloride was freed from iron after oxidation with chlorine by addition of manganese carbonate and recrystallization. In the case of barium chloride, the anhydrous salt was weighed out. In all the other cases the solution was diluted to exactly 0.2 normal with the help of a standard silver nitrate solution.

The thallium was determined with potassium permanganate as described above. In the case of the copper, owing to the blue color, that metal had to be removed by the addition of sodium hydrate before proceeding with the titration. The results are given in Table VII.

TABLE VII. TlCl in the presence of $MgCl_2$, $CaCl_2$, $BaCl_2$, $MnCl_2$, $ZnCl_2$, and $CuCl_2$.

Salt added.	Its Concentration.	Solubility of TlCl.	Mean of Experiments.	Mean for the six salts.
None.	0	. . .	0.01612	. . .
$MgCl_2$	0.025	0.00901	0.00904	. . .
$MgCl_2$	0.025	0.00908	. . .	. . .
$CaCl_2$	0.025	0.00898	0.00899	. . .
$CaCl_2$	0.025	0.00899	. . .	. . .
$MnCl_2$	0.025	0.00897	0.00898	0.00901
$MnCl_2$	0.025	0.00898	. . .	. . .
$ZnCl_2$	0.025	0.00897	0.00899	. . .
$ZnCl_2$	0.025	0.00901	. . .	. . .
$CuCl_2$	0.025	0.00904	0.00905	. . .
$CuCl_2$	0.025	0.00907	. . .	. . .
$MgCl_2$	0.05	0.00617	0.00618	. . .
$MgCl_2$	0.05	0.00618	. . .	. . .
$CaCl_2$	0.05	0 00623	0.00624	. . .
$CaCl_2$	0.05	0.00626	. . .	. . .
$BaCl_2$	0.05	0.00618	0.00620	. . .
$BaCl_2$	0.05	0.00623	. . .	0.00620
$MnCl_2$	0.05	0.00617	0.00617	. . .

TABLE VII. TlCl in the presence of $MgCl_2$, $CaCl_2$, $BaCl_2$, $MnCl_2$, $ZnCl_2$, and $CuCl_2$.
(Continued)

Salt added.	Its Concentration.	Solubility of TlCl.	Mean of Experiments.	Mean for the six salts.
$MnCl_2$	0.05	0.00617	. . .	. . .
$ZnCl_2$	0.05	0.00627	0.00627	. . .
$ZnCl_2$	0.05	0.00627	. . .	. . .
$CuCl_2$	0.05	0.00613	0.00614	. . .
$CuCl_2$	0.05	0.00616	. . .	. . .
$MgCl_2$	0.1	0.00412	0.00413	. . .
$MgCl_2$	0.1	0.00413	. . .	. . .
$CaCl_2$	0.1	0.00416	0.00417	. . .
$CaCl_2$	0.1	0.00418	. . .	. . .
$BaCl_2$	0.1	0.00423	. . .	. . .
$BaCl_2$	0.1	0.00426	0.00425	. . .
$BaCl_2$	0.1	0.00426	. . .	0.00417
$MnCl_2$	0.1	0.00410	0.00412	. . .
$MnCl_2$	0.1	0.00413	. . .	. . .
$ZnCl_2$	0.1	0.00412	0.00412	. . .
$ZnCl_2$	0.1	0.00412	. . .	. . .
$CuCl_2$	0.1	0.00419	. . .	. . .
$CuCl_2$	0.1	0.00425	0.00422	. . .
$CuCl_2$	0.1	0.00422	. . .	. . .
$MgCl_2$	0.2	0.00273	0.00275	. . .
$MgCl_2$	0.2	0.00277	. . .	. . .
$CaCl_2$	0.2	0.00284	0.00284	. . .
$CaCl_2$	0.2	0.00284	. . .	. . .
$MnCl_2$	0.2	0.00287	0.00286	0.00283
$MnCl_2$	0.2	0.00284	. . .	. . .
$ZnCl_2$	0.2	0.00279	. . .	. . .
$ZnCl_2$	0.2	0.00279	0.00281	. . .
$ZnCl_2$	0.2	0.00286	. . .	. . .
$CuCl_2$	0.2	0.00291	0.00291	. . .
$CuCl_2$	0.2	0.00292	. . .	. . .

II. DISSOCIATION OF THE BIVALENT-METAL CHLORIDES.

The somewhat remarkable and unexpected conclusion may be drawn at once from these figures that all the chlorides here considered are very nearly equally dissociated. That, in spite of the rather large variations occurring in special cases, the effect of these different salts on the solubility is in reality essentially the same may be clearly seen from a close inspection of the results.

The mean value of the solubility for the six salts has therefore been used in the calculation of the dissociation by formula (5). The results are given in the following table:

TABLE VIII. Dissociation of $MgCl_2$, $CaCl_2$, $BaCl_2$, $MnCl_2$, $ZnCl_2$, and $CuCl_2$.

Concentration (y).	Dissociation ($100\alpha_1$).	Constant $= \frac{1-\alpha_1}{\alpha_1^3 m^2}$
0.0347	82.2	267
0.0567	77.6	150
0.1045	69.4	83
0.2030	61.5	40

In all the cases previously considered the added salts have been binary, and the dissociation could take place only in one way. In the case of these bivalent-metal chlorides, however, two ways of dissociation are possible. Taking calcium chloride as an example, it is evident that this can break up, either into three ions, according to the reaction, $CaCl_2 = \overset{++}{Ca} + \overset{-}{Cl} + \overset{-}{Cl}$, or into two, according to the reaction, $CaCl_2 = \overset{+}{CaCl} + \overset{-}{Cl}$;[1] or partly in the one way and partly in the other.

This consideration is of importance; for without it the values of the above table are misleading. All that is determined by the solubility effect is the number of free chlorine ions in the solution of the added salt; and by the percentages given in the second column, headed "dissociation," is to be understood that the added salt gives rise to

[1] The dibasic organic acids have been shown by Ostwald (Zeitschrift für physikalische Chemie, 3, 282) to dissociate in this way; that is, only one hydrogen atom splits off until a very high dilution is reached.

that number of chlorine ions which it would give rise to if dissociated into *three* ions to the extent stated; the true dissociation is greater than this, if dissociation into two ions simultaneously takes place; but its value is not determined by these experiments.

The question whether the dissociation takes place in the one way or in the other, or in both at the same time, is one of considerable interest, and can be answered by determining whether the dissociation-values obtained at the four different concentrations vary in accordance with the laws of dissociation. It is clear, in the first place, since the dissociation exceeds fifty per cent., that the dissociation into two ions cannot alone take place; and it remains only to determine which of the two cases occurs, dissociation into three ions alone, or dissociation into three ions accompanied by that into two. In the former case the laws of mass action require that

$$\begin{matrix} Cy^3a_1^3 = y\,(1 - a_1) \\ (Ca + Cl + Cl = CaCl_2) \end{matrix} \left[\text{ or } C = \frac{1 - a_1}{y^2 a_1^3} \right]$$

The values of this theoretical constant are given in the third column of the above table, and their variation is such that the assumption that a dissociation into three ions alone occurs is obviously untenable. The dissociation diminishes much too slowly with increasing concentration. The solutions of the chlorides RCl_2 probably contain, therefore, undissociated RCl_2 molecules, and three kinds of ions, namely: $\overset{++}{R}$, $\overset{-}{Cl}$, and $\overset{+}{RCl}$, and the last kind is present in considerable quantities in the more concentrated solutions.

Finally, concerning the degree of the dissociation, it may be remarked that it is not very different from that of the alkali chlorides; it is, however, a few per cent. less.

12. EXPERIMENTS WITH THALLOUS CHLORIDE AND CADMIUM CHLORIDE. DISSOCIATION OF THE LATTER.

Of the bivalent-metal chlorides which I investigated, all proved to be dissociated to practically the same extent, with the single exception of cadmium chloride. This was found to diminish the solubility of thallous chloride much less than the chlorides above considered. Its effect on the solubility is shown in the table upon the next page.

TABLE IX. TlCl in the presence of $CdCl_2$. Dissociation of $CdCl_2$.

Concentration (x).	Solubility of TlCl.	Mean.	Dissociation ($100\,a_1$).	Concentration (y).	Constant.
0	. . .	0.01612	. . .	. . .	. . .
0.025	0.01040	0.01040	59.7	0.0407	1140
0.025	0.01039	. . .	. . .	. . .	. . .
0.05	0.00780	0.00780	53.5	0.0628	773
0.05	0.00779	. . .	. . .	. . .	. . .
0.1	0.00576	0.00578	43.1	0.1111	577
0.1	0.00580	. . .	. . .	. . .	. . .
0.2	0.00429	0.00427	33.3	0.2098	410
0.2	0.00426	. . .	. . .	. . .	. . .

A comparison with the results of Table VII shows that a 0.2 normal solution of cadmium chloride reduces the solubility of thallous chloride to about the same extent as a 0.1 normal solution of the other bivalent-metal chlorides. Both contain, therefore, nearly the same number of chlorine ions; or the dissociation of cadmium chloride in 0.2 normal solution is approximately one half that of the other chlorides in 0.1 normal solution. With increasing dilution, however, the dissociation of the former increases much more rapidly than that of the latter, as was to be expected.

The figures of the last column of the above table, which are the values of the theoretical constant $\frac{1 - a_1}{a_1^3 y^2}$ corresponding to the dissociation into three ions, indicate that this manner of dissociation is not the only one, and it is highly probable that besides cadmium and chlorine ions, compound ions CdCl are also present in the solution.

Considering the many similarities between the elements cadmium and zinc, and their positions in the periodic system, it might well have been expected that zinc chloride would at least occupy an intermediate place between cadmium and the alkaline earth chlorides in respect to its dissociation. We have seen, however, that this was not the case, zinc chloride not differing appreciably in its solubility effect from the alkaline earth chlorides — a fact again established by the experiments with lead chloride described below.

I must not fail to mention that these conclusions concerning the

relative dissociation of the bivalent-metal chlorides are identical with those to which consideration of the electrical conductivity of their solutions leads. For example, according to the experiments of Kohlrausch[1] and Wershoven,[2] the ratio of the molecular conductivity of a 0.1 normal solution to that of an extremely dilute one is 0.75 in the case of barium chloride, 0.74 in that of zinc chloride, and 0.42 in that of cadmium chloride, thus practically identical in the first two cases and greatly less in the last.

Finally in this connection, let me call attention to the experiments of Tammann[3] on the diminution of the vapor pressure of water at 100° by these chlorides. The values of this in millimeters in the case of solutions containing one molecule of salt in two kilograms of water were as follows:

KCl	$BaCl_2$	$SrCl_2$	$CaCl_2$	$MgCl_2$	$ZnCl_2$	$CdCl_2$
12.2	16.4	16.8	17.0	16.8	9.2	9.6

While the solubility and conductivity experiments place beyond doubt the fact that zinc and the alkaline earth chlorides are nearly equally dissociated at ordinary temperatures, these results of Tammann seem to justify the interesting conclusion that at the boiling temperature the degrees of dissociation of zinc and *cadmium* chlorides are equal. Solubility experiments with these chlorides at higher temperatures would therefore be of great interest.

13. EXPERIMENTS WITH THALLOUS CHLORIDE AND MERCURIC AND LEAD CHLORIDES.

As mercuric chloride is known from its feeble conductivity to be very little dissociated, its effect on the solubility of thallous chloride might be expected to be nearly inappreciable. But the great tendency of this compound to form double salts manifests itself in this case also, and makes the principles of solubility effect inapplicable. When a solution of mercuric chloride is added to one of thallous chloride, a leafy white precipitate immediately forms, which has been shown by Jörgensen[4] to have the composition $HgCl_2$, TlCl.

As I intended to try the effect of the various chlorides on the solubility of lead chloride, the effect of this last on that of thallous

[1] Annalen der Physik und Chemie, 26, 195.
[2] Zeitschrift für physikalische Chemie, 5, 495, 507.
[3] Zeitschrift für physikalische Chemie, 2, 42.
[4] Journal für praktische Chemie, (2) 6, 86.

chloride was of especial importance. Experiments to determine this were repeatedly tried with utterly discordant results. This led to an investigation of the precipitate formed by the addition of lead chloride to a solution of thallous chloride, and this precipitate was found to consist not of the latter alone, but of a double salt of the two. The salt, which separated in thin pearly scales, was recrystallized from a half saturated solution of lead chloride, in order to free it from excess of thallous chloride, and then from hot water. It is but very little soluble in cold water, but more readily in hot. It gave on analysis results as follows:

	Found.	Calculated for $PbCl_2$, 3TlCl.
Pb	21.78	20.76
Tl	60.15	61.46
Cl	17.96	17.78
	99.89	100.00

Its composition corresponds most closely to the formula $PbCl_2$, 3TlCl. The salt was apparently not pure, though recrystallization did not appreciably change the percentage of chlorine. As for my present purpose it was of importance only to prove the existence of a double salt, in order to explain the anomaly in the solubility effect, I did not further follow the study of its composition. This example illustrates the second above-mentioned (page 261) cause of inapplicability of the principles of solubility effect, and shows, conversely, how an anomaly in the latter may lead to the discovery of new double salts.

14. EXPERIMENTS WITH LEAD CHLORIDE AND POTASSIUM, MAGNESIUM, CALCIUM, MANGANESE, ZINC, AND CADMIUM CHLORIDES, AND HYDROCHLORIC ACID.

In order to confirm the values of the dissociation arrived at above, and to eliminate any error that might arise in individual cases from the occurrence of special chemical action between the dissolved substances, such as we have just seen to occur between lead and thallous chloride, an entirely independent series of experiments was made, in which the effect of some of the same chlorides on another difficultly soluble chloride, namely that of lead, was investigated.

The saturation of the solutions was effected in the same manner as with thallous chloride. The amount of lead dissolved was determined volumetrically by adding an excess of standard potassium

bichromate, filtering, and, after addition of potassium iodide and sulphuric acid, titrating back with sodium thiosulphate with the aid of carbon bisulphide as indicator. The method gave excellent results.

The following table contains the results of these experiments:

TABLE X. $PbCl_2$ in the presence of HCl, KCl, $MgCl_2$, $CaCl_2$, $MnCl_2$, $ZnCl_2$, and $CdCl_2$.

Salt added.	Its Concentration.	Solubility of $PbCl_2$.	Mean of Experiments.
None.	0	0.07773	. . .
None.	. . .	0.07771	. . .
None.	. . .	0.07765	0.07770
None.	. . .	0.07773	. . .
None.	. . .	0.07767	. . .
HCl.	0.05	0.04786	0.04786
HCl.	0.05	0.04786	. . .
HCl.	0.1	0.03244	0.03243
HCl.	0.1	0.03242	. . .
HCl.	0.2	0.01923	0.01927
HCl.	0.2	0.01932	. . .
KCl.	0.05	0.0480	. . .
KCl.	0.05	0.0481	0.0482
KCl.	0.05	0.0487	. . .
KCl.	0.1	0.0341	0.0341
KCl.	0.1	0.0342	. . .
KCl.	0.2	0.0218	. . .
KCl.	0.2	0.0219	0.0219
KCl.	0.2	0.0220	. . .
$MgCl_2$.	0.05	0.0503	0.0503
$MgCl_2$.	0.05	0.0503	. . .
$MgCl_2$.	0.1	0.0350	0.0350
$MgCl_2$.	0.1	0.0350	. . .
$CaCl_2$.	0.05	0.0499	. . .
$CaCl_2$.	0.05	0.0503	0.0503
$CaCl_2$.	0.05	0.0506	. . .
$CaCl_2$.	0.1	0.0354	0.0355

TABLE X. $PbCl_2$ in the presence of HCl, KCl, $MgCl_2$, $CaCl_2$, $MnCl_2$, $ZnCl_2$, and $CdCl_2$.
(Continued)

Salt added.	Its Concentration.	Solubility of $PbCl_2$.	Mean of Experiments.
$CaCl_2$.	0.1	0.0357	. . .
$CaCl_2$.	0.2	0.0218	. . .
$CaCl_2$.	0.2	0.0220	0.0219
$CaCl_2$.	0.2	0.0220	. . .
$MnCl_2$.	0.05	0.0501	0.0501
$MnCl_2$.	0.05	0.0500	. . .
$MnCl_2$.	0.1	0.0347	. . .
$MnCl_2$.	0.1	0.0349	0.0349
$MnCl_2$.	0.1	0.0349	. . .
$MnCl_2$.	0.2	0.0217	. . .
$MnCl_2$.	0.2	0.0217	0.0217
$MnCl_2$.	0.2	0.0218	. . .
$ZnCl_2$.	0.2	0.0220	0.0220
$ZnCl_2$.	0.2	0.0220	. . .
$CdCl_2$.	0.05	0.0600	0.0601
$CdCl_2$.	0.05	0.0603	. . .
$CdCl_2$.	0.1	0.0480	0.0481
$CdCl_2$.	0.1	0.0482	. . .
$CdCl_2$.	0.2	0.0355	0.0355
$CdCl_2$.	0.2	0.0355	. . .

A consideration of these results gives rise to the following conclusions:

1. The four chlorides of magnesium, calcium, manganese, and zinc, exert practically the same effect on the solubility of lead chloride.

2. Potassium chloride reduces its solubility to a slightly greater extent than the bivalent-metal chlorides just mentioned.[1]

3. Hydrochloric acid diminishes its solubility to a somewhat greater extent than potassium chloride.

[1] Except in the case of the most concentrated solution (0.2 normal), where it reduces it by the same amount.

4. A 0.2 normal cadmium chloride solution dissolves the same quantity of lead chloride as a 0.1 normal solution of the other bivalent-metal chlorides.

Each of these statements has been shown above[1] to hold true in the case of thallous chloride, so that in every particular the relative influence of the various chlorides on the solubility of thallous and lead chlorides is the same. This result is an evident consequence of the theory of solubilty effect, and it greatly confirms the conclusions above drawn in regard to the dissociation of the chlorides. For if in a particular case any disturbing cause, like the formation of a double salt, made the principles of solubility effect inapplicable, it is improbable that that same cause would exist, and especially so that it would exert the same influence, in the case of two dissimilar substances like lead and thallous chloride. If, for example, the fact that a 0.2 normal solution of cadmium chloride exerts the same influence as a 0.1 normal solution of calcium chloride on the solubility of thallous chloride is due not to the smaller dissociation of the former, but to the formation of a double salt between it and thallous chloride, then the behavior of cadmium and calcium chlorides towards lead chloride can be explained only by assuming likewise the formation of a double salt between lead and cadmium chlorides; and it is, moreover, necessary to assume its formation in quantity just sufficient to produce the strange coincidence in question — an occurrence so improbable as to be scarcely worthy of consideration.

It is possible, however, in the case of the bivalent-metal chlorides to show quantitatively that the experiments with lead chloride lead to nearly the same values of the dissociation as those with thallous chloride. This I will now proceed to do, in the converse manner, however, by showing that the solubility of lead chloride calculated from the values of the dissociation deduced from the thallous chloride experiments agrees with that found experimentally.

15. THEORY OF SOLUBILITY EFFECT BETWEEN TWO TERNARY ELECTROLYTES.

Up to the present time no attempt has been made to extend the principles of solubility effect to the case where the substance with which the solution is saturated breaks up into three ions. Therefore, although the experiments contained in the foregoing table were made with the special purpose of confirming the dissociation values obtained

[1] Pages 278, 279, 273, and 280.

with thallous chloride, it has been thought best in what follows to make use of them as examples of the theory of solubility.

In the attempt to calculate the solubility of lead chloride in the presence of other chlorides, two difficulties are met with. First, we have no direct knowledge of the dissociation of lead chloride. As stated above, I had hoped to determine this by studying its influence on the solubility of thallous chloride; but this failed, owing to the formation of a double salt between them. We are helped out of this difficulty, however, by the fact above shown, that all the investigated bivalent-metal chlorides, with the single exception of that of cadmium, are equally dissociated, so that it is highly probable that lead chloride agrees with them in this respect. The second difficulty is more serious, and arises from the above demonstrated probability that the bivalent-metal chlorides dissociate in two ways, both into two ions and into three; lead chloride, for example, on the one hand, into $\overset{+}{PbCl}$ and $\overset{-}{Cl}$, and on the other into $\overset{++}{Pb}$, $\overset{-}{Cl}$ and $\overset{-}{Cl}$. As long, however, as the solutions are not too concentrated, the quantity of PbCl ions cannot be large, and a tolerably good agreement with the facts may be expected from the assumption that the dissociation into three ions alone takes place.

The condition of equilibrium existing between lead chloride and its dissociation products is represented by the following equation, in which the symbols stand for the active masses of the substances represented by them, and C_1 and C_2 are dissociation constants:

$$PbCl_2 = C_1 \times \overset{++}{Pb} \times \overset{-}{Cl} \times \overset{-}{Cl} + C_2 \times \overset{+}{PbCl} \times \overset{-}{Cl}.$$

By the fundamental principle of solubility effect the amount of undissociated lead chloride in the saturated solution is invariable, whatever other salt may be simultaneously present. The right hand member of the equation must, therefore, also be a constant quantity. This member becomes now much simplified from the relation required by the laws of mass action,

$$\overset{+}{PbCl} = C_3 \times \overset{++}{Pb} \times \overset{-}{Cl},$$

which holds true since the amount of undissociated $PbCl_2$ is constant.[1] It follows, therefore, that

$$\overset{++}{Pb} \times \overset{-}{Cl} \times \overset{-}{Cl} = \text{a constant.}$$

That is to say, even under the assumption that the dissociation takes

[1] Otherwise the quantity of $\overset{+}{PbCl}$ would be a function of that as well.

place in both possible ways, it is nevertheless strictly true that the product of the active mass of the lead ions into the square of the active mass of the chlorine ions is a constant quantity. Difficulty arises, however, in the practical application of this principle, if we admit the existence of PbCl ions; for in that case we have no means of knowing the quantity of lead ions present in the saturated solution of lead chloride in pure water. But if we assume that the number of lead ions is one half the number of the chlorine ions, the quantity of which has been shown by the experiments with thallous chloride, all necessary data become known.

Under that assumption, the constancy of the product of the active masses of the lead and chlorine ions is expressed in the case that the added salt is also ternary by the formula,

$$m_0^3 a_0^3 = ma\,(ma + xa_1)^2,$$

in which the letters have the same significance as before (see page 261). When the two salts are equally dissociated in equally concentrated solutions, $a = a_1$, and its value is that corresponding to the concentration $m + x$ of either salt alone, as may easily be shown in a manner similar to that on page 269. The above equation may then be written

$$m^3 + 2xm^2 + x^2m - \left(\frac{m_0 a_0}{a}\right)^3 = 0.$$

This equation has evidently but one positive root. The algebraic solution is of no assistance in determining its value, but it is easily calculated in each special case by Horner's method, after substitution of the numerical values of x, m_0, a_0, and a. This formula should represent the effect of magnesium, calcium, zinc, and manganese chlorides on the solubility of lead chloride. The values of the dissociation a_0 and a used in calculating the theoretical solubility m were obtained by graphic interpolation from those of Table VIII. The full data are as follows:

$m_0 = 0.0777$; $a_0 = 0.733$: for $x = 0.05$, $a = 0.697$; for $x = 0.1$, $a = 0.661$; and for $x = 0.2$, $a = 0.605$.

These calculated solubilities are placed in the following table by the side of the mean values of the results experimentally obtained with the chlorides of magnesium, calcium, zinc, and manganese.

TABLE XI. Calculated and actual solubility of $PbCl_2$ in presence of $MgCl_2$, $CaCl_2$, $ZnCl_2$, and $MnCl_2$.

Added Quantity (x).	Actual Solubility.	Calculated Solubility.
0.00	0.0777	. . .
0.05	0.0502	0.0522
0.10	0.0351	0.0351
0.20	0.0218	0.0176

In the case of the two diluter solutions, the agreement between the theoretical and experimental values is quite satisfactory; in the case of the most concentrated one, the divergence is, as was to be expected, somewhat larger. These results deserve consideration, I think, not only for their bearing on the phenomena of solubility, but also as an extension of the principles of mass action to a case of dissociation not previously studied. One example of dissociation into three components has indeed been given by Naumann and Horstmann, namely, that of ammonium carbamate into ammonia (2 mols.) and carbonic dioxide. This was, however, a condition of equilibrium between a solid body and its dissociation products, and not one in a homogeneous system between the undissociated and dissociated portions of a compound.

In the case of the other chlorides whose effect on the solubility of lead chloride was tried, namely, cadmium and potassium chlorides and hydrochloric acid, the calculation of the solubility becomes very complicated; for the assumption that the added salt and that with which the solution is saturated are equally dissociated no longer holds. Even if practicable, this calculation would be hardly necessary; for, a pretty close agreement between theory and fact having been shown to exist in the case of the chlorides of magnesium, calcium, zinc, and manganese, it follows from the facts discussed on page 284, that the same must hold true in the case of the other chlorides.

16. EXPERIMENTS WITH LEAD CHLORIDE AND MERCURIC CHLORIDE AND LEAD NITRATE.

As lead chloride is one of the few bivalent-metal chlorides with which mercuric chloride has not been shown to form a double salt, I thought that possibly one did not exist, and that the effect of mercuric

chloride on the solubility of lead chloride might be in accordance with the principles of solubility effect. The experiments proved, however, that such was not the case; for a considerable increase in the solubility took place, indicating the existence of a soluble double compound.

Lead nitrate, whose effect on lead chloride was studied with the idea of obtaining some information concerning the number of free lead ions in its solution, was found to act in the same way, causing an increased instead of a diminished solubility.

The results of two experiments made with these substances are given in the following table:

TABLE XII. $PbCl_2$ in the presence of $HgCl_2$ and $Pb(NO_3)_2$.

Salt added.	Its Concentration.	Solubility of $PbCl_2$.	Mean Solubility.
None.	0	0.0777	0.0777
$HgCl_2$	0.1	0.0997	0.0992
$HgCl_2$	0.1	0.0987	. . .
$Pb(NO_3)_2$	0.2	0.0824	0.0832
$Pb(NO_3)_2$	0.2	0.0841	. . .

17. CONCLUSION.

It seems desirable at the close of this somewhat lengthy article to recapitulate the more important results contained in it. The investigation was undertaken with the general purpose of showing that solubility experiments furnished us with a new and quite accurate method of determining the dissociation of salts, and with the special purpose of applying this method in order to obtain information concerning the dissociation of the chlorides of the various elements.

The effect of eleven different chlorides on the solubility of thallous chloride was investigated, each in four distinct concentrations, and the dissociation of each was calculated by means of the principles of solubility effect. The three alkali-metal chlorides, potassium, sodium, and ammonium, were found to be equally dissociated, their dissociation increasing from sixty-five per cent. at 0.2 to eighty-nine per cent. at 0.03 normal. Hydrochloric acid did not differ very greatly from these; it was, however, a little more dissociated, the values varying from seventy-three per cent. to eighty-nine per cent. between the

named concentrations. With a single exception all the bivalent-metal chlorides investigated, namely those of magnesium, calcium, barium, zinc, manganese, and copper, proved, like the univalent metal chlorides, to be equally dissociated — a rather remarkable fact when the dissimilarity of the elements is considered. Their dissociation is at all four concentrations about five per cent. less than that of the alkali chlorides. Cadmium chloride forms the exception spoken of; its dissociation is very much less than that of the other bivalent-metal chlorides, being at the four concentrations, in its case 33, 43, 53, and 60 per cent., while it is 62, 69, 78, and 82, in the case of the latter chlorides.

In confirmation of these results it was then shown that these different chlorides have the same relative effect on the solubility of lead chloride as on that of thallous chloride, thus eliminating the possible error that might arise in individual cases from the formation of double salts. And in the case of the bivalent-metal chlorides, the correctness of the dissociation values deduced from the thallous chloride experiments was proved almost conclusively by showing that the solubility of lead chloride was diminished to the extent corresponding to those values. The solubility of the lead chloride in the presence of the other chlorides was not calculated; but it follows from the fact just stated concerning the relative effect of the various chlorides that the two series of experiments (with thallous and lead chlorides) lead, in their case also, to practically the same values of the dissociation.

As the salts were investigated at four independent concentrations, it was possible to determine the manner in which the dissociation varies with the dilution; and one of the most important results arrived at is the proof that the univalent-metal chlorides follow the laws of dissociation in this respect. These laws are not followed, however, if the dissociation values deduced from the electrical conductivity are assumed correct; and this fact formed one of the few anomalies which the dissociation theory had not been able to explain. A comparison of the dissociation deduced from the solubility effect with that deduced from the electrical conductivity showed very considerable differences, especially in the case of the strong solutions. These experiments serve, therefore, to confirm the conclusion already drawn in my previous article that the electrical conductivity does not furnish an accurate measure of the dissociation except in cases where the number of ions present in the solution is very small, as in the case of weak organic acids.

The bivalent-metal chlorides were not found to obey the laws of mass action governing dissociation into three components, even approximately; and this led to the conclusion that dissociation into two ions, according to the reaction, $MCl_2 = \overset{+}{M}Cl + \overset{-}{Cl}$, probably accompanies that into three — a conclusion, however, of which further confirmation is desirable.

Finally, there should be mentioned the close agreement of the calculated with the actual solubility of lead chloride in the presence of other bivalent-metal chlorides; for in this calculation is involved an extension of the principles of mass action to a kind of dissociation not previously studied; and by it is contradicted what I believe to be a fundamental error in the statement of those principles. I refer, namely, to the so generally adopted statement of the law of Guldberg and Waage, that chemical action is proportional to the number of *equivalents* of each of the reacting substances in the unit of volume. Instead of "equivalents" should be inserted, I think, "molecules." The case above studied makes clear the fundamental difference between the two statements. The former would require that in a saturated solution of lead chloride the product of the number of equivalents of lead into the number of equivalents of chlorine be constant, while the latter requires that the product of the number of lead molecules into the *square* of the number of chlorine molecules be constant. This single example is by no means sufficient to establish the general principle, and it is very desirable to bring further experimental proof of it.

In conclusion, I desire to express my great obligation to my friend Dr. S. P. Mulliken, by whom many of these experiments were carried out.

TECHNOLOGY QUARTERLY.

VOL. IV. DECEMBER, 1891. NO. 4.

THE PLACE OF SCIENTIFIC AND TECHNICAL SCHOOLS IN AMERICAN EDUCATION.[1]

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AMONG the vast changes in the spirit and life of our country, in the arts, the industries, the ideas, the aspirations, of the American people, which were brought about by, or which coincided with, the great struggle from 1861 to 1865, none is more remarkable than the rapid development of schools of applied science and technology. It is no part of my duty to name even the most important of these, or to attempt to divide among them the honor of what they have, as a whole, achieved. I shall confine myself to accounting, as far as I may, for the rapidity with which these schools have spread over the land, and to estimating their place in our educational system.

The nearest and easiest thing to say regarding the growth of scientific and technical schools, since the fortunate conclusion of the civil war, is that the industrial development of the country had reached the point where it had become necessary that the enterprises into which our labor and capital were to be put should be organized and directed with much more of skill and scientific knowledge than had been applied to our earlier efforts at manufactures and transportation; and so, in the fullness of time, scientific and technical schools came. In this view there is much of truth. The vaster enterprises of these later days, the ever increasing possibilities of modern commerce and industry, the intensifying severity of competition due to quickened com-

[1] Address at the Convocation of the University of the State of New York, Albany, July 9, 1891.

munication, fast mails, cheap freights and ocean cables, had indeed created an urgent want for greater technical skill and more highly trained intelligence. The old wasteful ways of dealing with materials, the rule-of-thumb methods of construction, the haphazard administration, characteristic of our earlier industrial efforts, could not have been continued without greatly retarding the national development and without irreparable loss in the result. But not, at the time spoken of, had this want become one of which our people were generally conscious; much less had it created a demand for such institutions which would of itself have sufficed to bring them into existence. The establishment of scientific and technical schools in the United States was to constitute a striking instance of the principle that, in some things, supply must create demand.

Economists and people generally are so much accustomed to think of the more usual condition in which demand creates supply, that they often forget — indeed, to many it never occurs — that there is another large class of cases, and these far the most important of all, in which the opposite rule obtains. In the lower ranges of life, in matters of clothing, food and shelter, and indeed in holding on to whatever advances civilization has once fairly and fully made, whether in material or in higher things, the conscious wants of humanity will in all ordinary cases suffice to secure the due supply, without any organized public or private effort other than that originating in personal interest. But in all things high and fine, and generally also in every advance which material civilization is to make, there must be a better intelligence than that of the market, which shall apprehend, not what the people want, but what they ought to want; there must be disinterested efforts on the part of the natural leaders of society, which shall secure, at whatever sacrifice, such a demonstration of the merits and advantages of the yet unknown thing, such a supply of the new good, as shall create the demand for it. It will not be until that want has been fairly and fully wrought into the public consciousness, that the supply may thereafter be left to take care of itself.

The American schools of technology illustrate in an eminent degree the law of human progress which I have stated. These schools did not come into existence in obedience to a demand for them. They were created through the foresight, the unselfish devotion, the strenuous endeavors of a few rich men, and of many very poor men, known as professors of mathematics, chemistry, physics, and geology. At the

time they came into existence there was a smaller demand for technically trained men than there is today, when for twenty-five years these schools have been pouring out their hundreds of graduates annually. That demand has been created by first furnishing the supply; by showing what young men properly educated and highly trained can do in organizing and directing the forces of American industry.

That these schools, in spite of the fact that they had everything to do at once and little to do it with; in spite of the fact that they had no traditions to govern them, and had, indeed, the whole philosophy of their subject to evoke, *a priori;* in spite of general public indifference and even of much contempt, have done their work exceedingly well, even from the first, is fairly implied in the foregoing statement. It is truly remarkable that, with so little to go by and so much to do, all at once, out of such scanty means, there should have been so little waste of effort, so little done injudiciously, so few steps taken that needed to be retraced.

Credit should also be given to the Congress of the United States for the act which was passed July 2, 1862, under the enlightened leadership of the Hon. Justin S. Morrill, of Vermont, making generous provision for the establishment, in the several States, of colleges of agriculture and the mechanic arts. Most of this provision was, it is true, devoted to the creation of agricultural schools, regarding which the scope of this address does not require me to speak, and, indeed, regarding which I should scarcely presume to express an opinion; yet the part which was assigned to the promotion of the mechanic arts proved to be a most valuable and timely reënforcement of the American system of technological schools.

But no one who thoroughly believes in the mission of schools of this class can be content merely to assert that the full time had come in the economic evolution of the nation when such schools were imperatively needed for the promotion of our industries, and that the institutions thus called into being have done this, their primary work, with triumphant success. We go far beyond this, and assert for these schools that they have come to form a most important part of the proper educational system of the country, and that they are today doing a work in the intellectual development of our people which is not surpassed, if indeed it be equaled, by that of the classical colleges. No statement less broad and strong than this would begin to do justice to the view we take of what these schools are now doing, and are in an

increasing measure to do, for the manhood and citizenship of the country. We believe that in the schools of applied science and technology as they are carried on today in the United States, involving the thorough and most scholarly study of principles directed immediately upon useful arts, and rising, in their higher grades, into original investigation and research, is to be found almost the perfection of education for young men. Too long have we submitted to be considered as furnishing something which is, indeed, more immediately and practically useful than a so-called liberal education, but which is, after all, less noble and fine. Too long have our schools of applied science and technology been popularly regarded as affording an inferior substitute for classical colleges to those who could not afford to go to college, then take a course in a medical or law school, and then wait for professional practice. Too long have the graduates of such schools been spoken of as though they had acquired the arts of livelihood at some sacrifice of mental development, intellectual culture, and grace of life. For me, if I did not believe that the graduates of the institution over which I have the honor to preside were as well educated men, in all which the term "educated man" implies, as the average graduate of the ordinary college, I would not consent to hold my position for another day. It is true that something of form and style may be sacrificed in the earnest, direct, and laborious endeavors of the student of science; but that all the essentials of intellect and character are one whit less fully or less happily achieved through such a course of study, let no man connected with such an institution for a moment concede!

That mind and manhood alike are served in a preëminent degree by the systematic study of chemistry, physics, and natural history, has passed beyond dispute. The haste with which the colleges themselves are throwing over many of their traditional subjects to make room for these comparatively new studies, shows how general has become the appreciation of the virtue of these, when combined with laboratory methods, as means of intellectual and moral training.

I have spoken of the characteristic studies of these schools as the best of all available means of both moral and intellectual training. I believe this claim to be none too broad.

1. The sincerity of purpose and the intellectual honesty which are bred in the laboratory of chemistry and physics, stand in strong contrast with the dangerous tendencies to plausibility, sophistry, casuistry, and self-delusion which so insidiously beset the pursuits of metaphys-

ics, dialectics, and rhetoric, according to the traditions of the schools. Much of the training given in college in my boyhood was, it is not too much to say, directed straight upon the arts which go to make the worse appear the better reason. It was always an added feather in the cap of the young disputant that he had won a debate in a cause in which he did not believe. Surely, to an audience in these more enlightened days it is not needful to say that this is perilous practice, if, indeed, it is not always and necessarily pernicious. Even where the element of purposed and boasted self-stultification was absent, there was a dangerous and a mischievous exaltation of the form above the substance of the student's work, which made it better to be brilliant than to be sound.

Contrast with this the moral and intellectual influence of the studies and exercises we are considering. The student of chemistry or physics would scarcely know how to defend a thesis which he did not himself believe. In that dangerous art he has had no practice. The only success he has hoped for has been to be right. The only failure he has had to fear was to be wrong. To be brilliant in error only heightened the failure, making it the more conspicuous and ludicrous. How wholesome to the mind and heart of the pupil is such a regimen!

2. Again, in addition to the graces of sincerity and intellectual honesty, which are the proper traits of physical and natural science; altogether aside, too, from any future technical uses to which the arts or the information acquired may be put, there is great virtue, as training for practical work in life, of whatever kind, in whatever sphere, to be found in the objective study of concrete things, which so largely makes up the curriculum of the schools we are considering.

3. Still another advantage which we claim for the characteristic studies of the new schools, is that, in a very large degree, they dispense with the system of examinations which has become the curse of modern education. The recent remarkable outburst in England, from educators of every name and class, against that system, justifies the strong terms I have used. It is admitted on all sides to be a problem of the greatest difficulty so to adjust the scheme of examinations that they shall not largely neutralize the good effects of sincere and straightforward study.

So far has cramming been carried in English universities, and even in our own colleges, that examinations have largely ceased to be a test

of the scholar's attainments, much more of his real proficiency in his studies. Students who have a marked facility in this sort of thing acquire, in time, the faculty of passing creditably examinations on matters of which they know almost absolutely nothing. By steadily cramming for a few days and nights, under artful coaches, who know the professors' weaknesses and fads, a young man exceptionally expert can "get up" a subject,[1] of which he would be troubled, the morning after examination, to give an intelligible account. A special organ — the examination organ — becomes developed, which is as specific as the water-sacks attached to the stomach of a camel, intended only to carry a certain amount of refreshment over a very dry place for a very short time. Indeed, the comparison fails to do justice to its subject. The examination organ is at once as specific and as external as the pouch of a kangaroo.

From this serious difficulty, schools of applied science and technology are, by the very nature of the case, largely freed. Indeed, the inapplicability of the scheme of examinations to the studies we are considering has even been made an argument against their introduction into universities. Prof. Parsons Cooke, in addressing a body of students at Harvard recently, said: "When advocating in our mother university of Cambridge, in Old England, the claims of scientific culture, I was pushed with an argument which had very great weight with the eminent English scholars present, and which, you will be surprised to learn, was regarded as fatal to the success of the natural science triposes then under debate. The argument was, that the experimental sciences could not be made the subjects of competitive examinations."

It is not true that chemistry and physics cannot be made the subject of examinations AFTER THEIR KIND; but it is true that, under competent teachers of these sciences, examinations have far less of the character of a cram, and far more of the character of a test of ability to do work. Moreover, in such a scheme of instruction, as a whole, examinations perform a much less important part, while the daily and

[1] I would not disparage the importance, as a professional accomplishment, of the ability to "get up" a subject in a very short time under high pressure. A lawyer has often occasion to do this very thing. But this is a professional accomplishment, and should be acquired as such. The period of professional study is not too late for the acquirement of this faculty. It can even be acquired later still, in the course of professional work. Such practice, however, in my judgment, forms no part of general education and training, and is only vicious and mischievous in the culture stage.

weekly exercises in the laboratory become continually of more and more account as a means of ascertaining the scholar's real progress. In this the schools of applied science and technology comply with the demands of modern thought in pedagogics. In no department of life more than in education is the Scripture precept of authority, "Let the dead bury their dead." The best examination which a student can pass is by showing his ability to *do the next thing.* If he can pass this examination successfully, the teacher need give little thought to what has gone before. And I venture, by the way, to suggest, with reference to the urgent inquiry now proceeding as to where the American youth loses two years of time in his preparation for college, whether a large part, if not the whole of that serious loss, is not sustained from the everlasting reviews and examinations through which the American teacher, alike in the primary, the grammar, and the high school, insists upon dragging the pupil three times a year or oftener, not only requiring him thus to be continually going over again ground once traversed, but, what is of more consequence, creating a sentiment throughout the school-room which inspires the scholars to be forever looking back, and not forward.

4. The last of the advantages attendant on scientific instruction which I shall enumerate, though the list might be still further extended, is found in a better relation between teacher and pupil. I would not willingly be guilty of exaggeration in this matter. With a really great and gifted teacher, the attitude of the scholar will always be that of respect and admiration, whether with or without affection and personal intimacy. But it cannot be denied that, in the traditional college, with the traditional subjects of instruction, the relation in question is likely to be less than a happy one. On the one side there is apt to be an undue assumption of knowledge, a tendency to dogmatism, and a too peremptory way of dealing with the pupil's doubts and difficulties. On the other side there is apt to be something of the tone of resistance, if not of resentment; a disposition to escape the teacher's scrutiny, if not to get around him with the petty tricks of the recitation room.

It would be foolish to assume that there is any virtue in the natural and physical sciences which will overcome the faults or deficiencies of mind and heart that are found in some teachers. There are men who will be priggish, pompous, and pretentious in doing anything. But there is a wonderful virtue in the studies we are considering for bringing teacher and scholar together in their work in a most simple, natu-

ral, and affectionate relation. He is the most successful teacher of science who puts himself in the attitude of discovering truth with his pupil, and of hunting with him for the object of their common search. Moreover, the very closeness of the contact in the laboratory of chemistry, physics, or mechanics, is such as to cause a continuous, insensible discharge of the electricity generated by the necessarily strict requirements of study and discipline, and thus to maintain the friendly relations of teacher and pupil, unbroken by those storms which sometimes gather and burst in colleges where the teacher sits, buttoned-up, on a platform, behind his desk, and lectures to his pupils from the chair of authority.

But it may be said: Considering all that may be claimed for the purely educational advantages of the scientific studies which run through the curriculum of the technological schools, why may not all these advantages be equally obtained by the student of the traditional college, and even to better effect, since there he may secure the pure gold of truth freed from the alloy of baser metal — by which term the critic would designate the useful, practical applications of science. It is here that it behooves us to take issue, most directly and aggressively, with those who assert for the old-fashioned colleges an educational virtue superior to that of the schools we represent. It is of the very essence of our case that the directness and immediateness of application to which the studies of our pupils are subject, under their very eyes and at their very hands, constitutes a tremendous educational force, securing a closeness and continuity of attention on the part of the pupil, an earnestness of effort, a zeal and enthusiasm of work, which it is utterly beyond the power of the teacher of classics or philosophy to arouse, except in the case of gifted students. If proof of this upon a large scale be needed, it is enough to refer to the well-known fact that law schools and medical schools invariably command the energies of their pupils in a far higher degree than do the colleges; and that hosts of young men who have idled and dawdled away the four years nominally devoted to classics and philosophy throw themselves with splendid enthusiasm into their professional studies when once they, for the first time, see upon what ends their efforts are directed, and how their energy and application are to promote their happiness and usefulness in life.

Even in the case of those young men who need no such incentive to secure their faithful attention and earnest endeavor, we yet hold

that schools of applied science and technology possess a distinct advantage, in that their students learn the truths of science in a somewhat different way, and, in the result, know them somewhat better than do those who study these truths, no matter how diligently, without immediate, direct, and constant reference to their applications. Without referring further at this point to the limitations and defects inherent in all academic systems of recitation and examination, I believe it to be true that the man who, in studying mathematics, for example, has only to look forward to a recitation tomorrow and an examination two weeks or two months hence, applies himself to the subject necessarily in a different spirit and also necessarily with an inferior result, in contrast with the man who, continually as he acquires his mathematics, puts it to use day by day in the laboratory of physics, mechanics, hydraulics, or steam engineering.

For these reasons we must decline to accept the characterization of the technical applications of science as the alloy which debases the pure gold of truth. We look upon them, the rather, as the tough, elastic bow which sends the keen shaft to its mark. And, be it remembered, zeal and enthusiasm of work are not to be valued merely because, or merely as, they secure directness of attention, continuity of application and sustained endeavor. In themselves, of themselves, they are in a high sense an educational force, telling immediately and telling powerfully upon intellect and character, contributing importantly to build up mental and moral substance firmly and healthily.

There is one school in the United States mainly devoted to the application of scientific principles to a professional art, which is so well known to all our people, and whose work in the development of mind and manhood has been so severely tested in the sight of the country and of the whole world, that I cannot forbear to allude to it here. I mean the Military Academy at West Point. There is no reason to believe that, for the thirty years preceding the civil war, the young men who went to that school were in any degree superior to those who entered Yale or Harvard. Indeed, there was at that time, at least throughout the North, a certain disinclination on the part of the more generous and ambitious of our youth to adopt the career of arms. Yet when the war broke out, what a wealth of intellect and character was displayed by the graduates of that one small school during the terrific trial to which they were instantly and without preparation subjected! Think how many men from that single academy, which had fewer

living graduates than either Amherst or Williams, led army corps and armies with distinction, on the one side or the other, in what was perhaps the greatest war of modern history! I said "of intellect and character," for it is character, even more than intellect, which enables the commander to bear the tremendous cares, responsibilities, and burdens of his office. What was it which, out of those few small classes of raw lads, developed a Grant, a Lee, a Sherman, a Meade, a Jackson, a Thomas, the two Johnstons, a Hancock, a Reno, a Reynolds, and a Sheridan, not to mention scores of others who "waxed valiant in fight" and commanded divisions and corps with a skill and address which have excited the admiration of the professional soldiers of Europe? Doubtless in some part it was the romance and the highly stimulating influences of the military career. Doubtless in part also it was the special inspiration of the tremendous occasion, fraught as that was with the destinies of a continent. But I believe it, in still greater part, to have been the perfectly natural effect of the application of perhaps not extraordinary powers to the thorough, patient, unremitting study of scientific principles, directed straight upon a worthy profession, under the tuition and guidance of renowned masters of that art, and under the constant influence of professional ideas, professional sentiments, and great professional examples.

A great deal more might be said in comparison of the influence of scientific teaching as carried on in the schools of applied science and technology, with the influence of the traditional, or of the more modern, modified, curriculum of the classical colleges; but perhaps enough has been said to justify the assertion that the former class of institutions are just as truly educational as the latter. Here I am content to rest my case. This conceded, let the youth of the land seek the one or the other kind of school, according to their individual tastes, predilections, and plans for life. I am far from being so bigoted as to suggest that there is not room enough in the educational system of the future for all the institutions of the elder type which have achieved for themselves a name in letters and philosophy; which have, with pains inexpressible, wrought out their own problems and created their own constituencies; and each of which has a host of eager, devoted alumni, ever turning gratefully to the halls in which they were nurtured, and delighting to give to the old college the fruit of their labors and the fruit of their loins. But I confidently look to see a largely disproportionate number of the new institutions which shall from time

to time come into being, built essentially upon the plan which has achieved such prodigious successes during the quarter-century now closing. Doubtless the present general scheme of the schools of technology will itself undergo considerable modification, alike from the results of added experience, from larger means, and from the infusion of a wiser and more generous spirit. Doubtless more of economic, historical, and philosophical studies will be introduced to supplement, by their liberalizing tendencies, the work of the sciences in making their pupils exact and strong. Possibly some ultimate form for institutions of the higher learning may yet be developed, which shall embody much of both the modern school of technology and of the old-fashioned college, with, perhaps, something taken from neither, but originating in the larger, fuller, riper life of a happier and richer future.

SOME CONSIDERATIONS REGARDING HELMHOLTZ'S THEORY OF CONSONANCE.[1]

By CHARLES R. CROSS and HARRY M. GOODWIN.

THE present paper contains the results of a number of experiments relating to certain aspects of the theory of consonance and dissonance put forth by Helmholtz in the *Tonempfindungen.* In that work the author gives as the number of beats producing the maximum of harshness from thirty to forty per second, reaching this conclusion from a consideration of the amount of dissonance of various different chords in different octaves.

Mayer (*American Journal of Science,* **108**, 241; **109**, 267) studied the relation of the number of beats causing the greatest harshness to the absolute pitch, and showed that there is a marked rise in this number as the pitch becomes higher. The beats studied by Mayer, however, as he himself pointed out, are in several respects different from those which occur in the case of mistuned unisons. They were caused by rotating a disc, which was provided with a circle of holes near the circumference, between a vibrating tuning-fork and a resonator, the number of beats per second being of course greater as the speed of the disc was greater. Hence the sound-wave reaching the ear was not at all a compound wave due to the superposition on one another of two simple sound-waves slightly differing in length, as in ordinary beats, but merely a series of simple waves with gaps, as it were; while the passage from maximum to minimum intensity in the sound reaching the ear was very different in its character from that actually occurring with ordinary beats. In referring to such beats as those studied by Mayer, we shall for the sake of brevity designate them as "interruptional beats," and the ordinary beats we shall call "interference beats."

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[1] Read at a meeting of the American Academy of Arts and Sciences, June 10, 1891.

We have thought it desirable to ascertain what results would be reached in different registers with interference beats, such as are present in actual dissonances. It was of course necessary to employ simple tones. Our method was a direct one, viz., to produce two such tones simultaneously, and to vary the pitch of one of these by known amounts, estimating by the ear when the sound became the harshest.

As a source of sound we employed the cylindrical resonators of Koenig, which were sounded by gently blowing across the opening a stream of air from the brass, slit-shaped "universal embouchure" made by the same maker. We found these tones to be very pure and loud, while their pitch could be varied with great readiness without altering the intensity of the sound emitted. The set of resonators employed ranged from Sol_1 to Mi_5, the ranges of successive resonators overlapping slightly. The two embouchures were connected to a *Y* by rubber tubes, and placed on adjustable supports in suitable relation to the apertures of the two resonators to be sounded. The strength of the blast was regulated by a stopcock, and the bore of each rubber tube could be closed more or less by a clamp, so that the force of the jet of air at each resonator could be adjusted with all needed delicacy. Thus the intensity of the notes given out by the two resonators could be adjusted so that they were exactly equal in loudness. This was done by causing the tones to beat slowly, and so adjusting the apparatus that there was apparently an absolute silence when the sound fell to its minimum intensity. No difficulty was found from any whirring of the sheet of air as it struck the edges of the aperture, since by proper adjustment this could be made quite inappreciable. Having once made the necessary adjustments the pitch of either note could be varied by drawing out or pushing in the lower half of the resonator. The actual pitch of the note given by the resonator was in all cases determined by comparison with a tuning-fork of known rate. As the pitch of the resonator when blown is to a certain extent dependent on the position of the embouchure and the strength of the blast, it would not be safe to assume that the actual note given by the resonator was that marked upon it. Neither resonator when sounded sensibly affected the pitch of the note given by the other.

The determination of the point at which the harshness of the dissonance produced by the tones of the two resonators reaches a maximum is of course a very difficult problem to be solved exactly. It is,

moreover, a question still to be investigated, whether precisely the same results would be reached by different observers possessed of equally good perception. The following figures given in Table I were ascertained from a long series of observations by one of the writers (Mr. Goodwin). They are liable to an error of a single beat in either direction, possibly somewhat more in one or two cases. They furnish, however, the first series of direct measurements of this kind which we know.

TABLE I.

Ut_2	Ut_3	Sol_3	Ut_4	Sol_4	Ut_5	Mi_5
19	26	29	32	36	41	45

It will be noticed that the result for Ut_4, 32 beats per second, agrees very closely with the number 33, which Helmholtz gives for maximum dissonance in the middle registers. He found that of the ordinary musical intervals, b' c'', as he designates it, was the most dissonant, and as this gave 33 beats per second, he considered this number as giving the harshest effect for tones of corresponding pitch.

The notes furnished by the resonators are also well adapted for the direct study of the limit at which the beats cease to be audible when their number is increased. As the interval between the beating notes is increased, the beats heard become fainter and fainter until they disappear. It is all but impossible to fix upon the precise point of disappearance, since the change in loudness of the beats is so gradual. But, as will presently appear, our results clearly show that the maximum number of beats perceptible, like the number giving the maximum dissonance, varies greatly with the pitch of the notes sounded.

The pitch of one of the resonators was varied so as to run upward and downward through the vanishing point of the beats. This procedure was extremely tiresome, and the values obtained were found to be considerably influenced by fatigue of the ear. The following table (Table II) gives the results obtained when the ear was in good condition. The first column gives the rate of vibration of the lower note, the next four the rate of the upper note as found in corresponding series of observations; the seventh column gives the number of beats, and the last the interval between the two notes.

TABLE II.

Lower Note.		Upper Note.				Mean.	Beats.	Interval.
		1.	2.	3.	4.			
Ut_2	128	175	170	177	173	174	46	Fourth +
Ut_3	256	328	321	326	329	326	70	Major third +
Sol_3	384	467	458	469	460	464	80	Major third — 16 vibrations.
Ut_4	512	620	616	618	611	616	104	Minor third (nearly).
Sol_4	768	882	890	886	890	887	119	Tone + $\frac{1}{4}$ tone (nearly).
Ut_5	1024	1160	1167	1160	1163	1163	139	Tone + 11 vibrations.
Mi_5	1280	1420	1424	1421	1418	1420	140	Tone — $\frac{1}{8}$ tone (nearly).

These figures abundantly confirm for interference beats the results already reached by Mayer for interruptional beats, viz., that the ear is capable of appreciating a greater number of beats as the pitch of the beating notes rises. They also show very beautifully the increasing consonance of small intervals as the pitch rises, noted by Helmholtz.

The numerical results reached by us differ, as a whole, considerably from those given by Mayer for interruptional beats in either of his papers, though the divergence is less than that between the two series representing his own observations and those of Mrs. Seiler. In some cases, however, the agreement is very close. Thus for Ut_2 we find the last trace of beats to occur when these are 46 in number. The number as observed by Mayer is 26, and as observed by Mrs. Seiler, 45. For Ut_3 our figure is 70, and Mrs. Seiler's is 70 also. For the other notes the divergence is wide, except for Ut_5, for which our results agree quite well with those obtained by Mayer himself. These differences may be due to the different way in which the variations of intensity in the beats progress, to the presence of pitch variations in interference beats, to the fact that in our method the musical interval of the beating notes increases as the beats increase in number, to mere differences in the estimation of the observers, or to some other less evident causes.

We find that the ratios between the number of beats producing the greatest harshness at different pitches, and the maximum number of beats discernible at those pitches, are as follows (Table III). The numbers given are obtained by dividing the former number by the latter.

TABLE III.

Ut_3	Ut_3	Sol_3	Ut_4	Sol_4	Ut_5	Mi_5
0.41	0.37	0.36	0.31	0.30	0.30	0.32

These figures, as a whole, agree very well with the ratio (four tenths) given by Mayer, although the absolute numbers whose ratios are taken are quite different from his. Our results also seem to indicate a clearly marked fall in this ratio as the pitch rises.

A few experiments were made using Professor Mayer's method, with some slight modifications, in order to observe the judgment of the same ear upon beats of different kinds. The tuning-fork employed was kept in vibration continuously by electricity, and the rotating disc was driven by an electro-motor, which secured great constancy of speed. We were seriously troubled by the sound made by the rotating disc, which produced a sort of siren effect as its openings passed before the aperture of the resonator. For Ut_3 the maximum number of intermittences perceived was 50 per second. For Ut_4 the number was about 106. These figures are liable to an error of perhaps two beats, or even a little more for the higher pitch. Mayer's figures are 47, 78, respectively, for his own observations on these notes, and 70, 130, for those of Mrs. Seiler. It is clear that there are very great differences in the sensitiveness of different ears for the perception of rapid beats.

In another series of experiments interruptions were produced by a break-wheel placed in a telephonic circuit with a magneto-telephone at each end. The sound of a tuning-fork was transmitted through the line, and any desired number of interruptions could be produced by varying the rate of rotation of the wheel. Considerable annoyance was experienced at times from sounds which apparently arose from microphonic action at the break-wheel. These can probably be avoided in future experiments by causing the wheel to divert the current from the receiver periodically, instead of interrupting it. The limits at which the beats disappeared, with the notes experimented upon, were as follows: Ut_4 48, Mi_4 56, Sol_4 69. These figures are the means of a number of observations, and are apparently true within less than one beat per second. Comparing this kind of interruptional beat with those given by Mayer's method, the maximum number perceptible seems to be smaller for the former than for the latter. This is probably due in part to the greater loudness of the sounds observed in the

latter case, but there is another marked difference which must exercise an important influence upon the phenomenon under consideration. With the beats produced by the break-wheel the sound begins and ends almost instantaneously, so that sharply marked intervals of sound and silence of equal duration succeed each other. One would naturally expect the telephonic beats to be the more distinct of the two, which is not the case. The subject is one which requires further study as to the influence of the relative duration of sound and silence in the telephonic method, and of the relative size of the apertures in Mayer's method.

In the matters which we have thus far discussed in the present paper, our results are fully in accordance with the theory of consonance proposed by Helmholtz. In some other particulars, however, we are led to conclude that this theory is incomplete, at least.

Some years ago in a paper read at the Philadelphia meeting of the American Association for the Advancement of Science, and of which an abstract was published in the Proceedings of that Society for 1884 (page 113), one of us called attention to the bearing of certain phenomena of binaural audition upon Helmholtz's theory, as follows:

"In connection with his study on the effects of beats in causing dissonance, Helmholtz considers the condition of the vibrating portions of the inner ear as to resonance and damping. In his remarks on the subject he assumes that no notes are capable of beating with each other unless they both affect the same vibrating element of the inner ear. This view of the matter gives a purely mechanical action in the ear as the explanation of the physiological phenomenon of beats.

"In addition to the experiments of Koenig, in which beats were obtained between notes of very great intervals, there are certain phenomena of binaural audition which appear to prove that this view is incorrect, and that rather than, or at least in addition to, this mechanical interference of vibrations in the ear there is a more obscure operation within the sensorium itself. I refer to the fact that beats may occur when the exciting sounds operate upon different ears, and also apparently between the after-sensation and the succeeding sound of mistuned unisons. Whether this last phenomenon can be observed when the after-sensation results from an impression made upon the opposite ear to that which is impressed by the succeeding sound, I do not know; but in view of the fact that two simultaneous sounds acting upon different ears may beat, I do not see why this result should not be possible.

"Now if beats arise between the after-sensation and a following sound, Helmholtz's view can be true only upon the supposition that the vibrating parts of the inner ear continue in motion as long as the after-sensation persists, and that there is no residual sensation capable of giving beats other than that which persists only as long as the actual vibration in the ear itself continues. Such a supposition seems quite improbable. Moreover, the phenomenon of beats in the case of sounds acting simultaneously upon different ears cannot be explained even upon this supposition. There must be some kind of *vibration*, using this term in the most general sense, or some kind of alternation of phase or state within the sensorium itself.

"There is no question as to the fact of the production of beats under the circumstances last mentioned. I have not only verified the fact by experiments conducted in the usual manner, but have also studied the beats produced when a tuning-fork is held close to one ear and the sound of a second fork, not quite in unison with it, is transmitted by telephone to the other, taking suitable precautions that the ear against which the receiving telephone was held was not affected by the sound of the neighboring fork. Beats were readily obtained, which grew weaker as the fork was moved away from the open ear and approached to the ear which was closed by the receiver. The same result occurred when, instead of mistuned unisons, harmonic forks were used, which gave beats with the fundamental."

In the experiments just cited, the fact that the fork gave feebler beats as it was moved towards the ear to which the telephone was applied seemed clearly to indicate that the beats were not likely to be due to sounds transmitted through the head from one ear to the other, or to the passage of the vibrations through the skull from the outer air. These experiments did not differ in other respects from those of previous observers. The beating of the sound persisting in one ear after excitation had ceased with a second sound falling upon the other ear was, in fact, observed by S. P. Thompson in 1881.

The existence of such binaural beats as are under consideration was noted as early as 1874 by Mach, who assumed that the sound was conducted from one ear to the other through the bones of the head. S. P. Thompson has studied the subject very carefully (*Philosophical Magazine*, **4**, 274; **6**, 383; **12**, 351; **13**, 406), and while he is evidently strongly inclined to believe that the phenomena are due to an interference of sensations not produced by the same ear, nevertheless

he does not appear to consider this as a fact finally settled beyond all question, although to us his results seem to leave small room for doubt on the matter.

The important bearing of the existence of binaural beats upon some parts of Helmholtz's theory of audition does not seem to have been very fully recognized. This may be because of some question as to whether each sound did not after all really act upon both ears, even when apparently applied to one alone. From the nature of the case it is very difficult to prove finally that there is no possibility of such conduction of sound from one ear to the other, but we have sought to study the phenomena of binaural beats under circumstances which render such conduction improbable in the highest degree. With this end in view we have repeated most of the experiments of Thompson, and made such additional tests as suggested themselves to us.

The sounds of two tuning-forks placed in a distant room were conveyed to the ears telephonically over two separate circuits. These forks were struck by hammers actuated by electro-magnets placed in circuits separate from those containing the telephones, and governed by keys operated by the foot of the observer at the receiving end of the line. This method was found to be preferable to running the forks continuously by electro-magnetic means, as the currents used in the latter method were liable to act directly upon the transmitting telephone and cause disturbing noises. Very powerful magneto-telephones were used as transmitters and receivers. Mi_4 forks were used throughout the experiments, unless otherwise stated, on account of the remarkable intensity with which their tones were transmitted, this being approximately the natural tone of the diaphragm. The singular phenomena of the localization in the back of the head of the sensation caused by two notes in opposite phases when heard by separate ears, and the apparent wandering of the sensation from one ear to the other in the case of binaural beats referred to by Thompson, were always clearly recognized.

The method of experimentation precludes any fusion of the two sounds, except within the head; but to decide whether the beats arise from an interference of the sensations or from a mechanical interference in Corti's organ, we must investigate further. We tried to throw some light on this point by the following experiments. It is well known that the vibrations of a fork, even when of too small amplitude to be heard when held opposite the ear itself, can be made

audible by pressing the stem of the fork against the bone of the skull, or, still better, against the teeth, the latter transmitting vibrations to the ears better than any other part of the head. A still more effectual means of making audible very small vibrations is to close the ear with a bit of beeswax and press the stem of the fork lightly against the wax. In this case the vibrations are transmitted to the membrana tympani by the small amount of air inclosed within the meatus, as is clear from the fact that the sound of the fork is heard on touching the wax long after it ceases to be audible on touching its stem to the pinna of the ear. Hence in this case there is no conduction to the middle or inner ear through the bones of the head. Whatever sound reaches the ear follows the ordinary path through the meatus to the membrana tympani. Now we found that the vibrations of a fork could be heard longer when touched to the wax in the ear than when held against the teeth. We therefore took two small tonometer forks making four beats per second, struck them very gently and held their stems against the teeth; loud beats were heard in the ears just as they are ordinarily heard when powerful forks are sounded in the air. The forks were then held in this position until the beats had entirely ceased to be audible, when they were removed, and the stem of each was touched to the wax closing the two ears. Instantly the two notes were heard, faintly but distinctly, in the ears to which they were held, and accompanying them were faint beats seeming to wander in the head from ear to ear, as is always the case with binaural beats. In trying this experiment care was taken that no vibration should be imparted to the fork from a gentle blow given on touching its stem to the wax, and it was found that the liability of such an occurrence could be greatly diminished by covering the end of the stem with a layer of wax.

To make sure that these beats were not the result of imagination we took forks giving four, eight, twelve, and sixteen beats per second at random, struck them lightly as before, but in this case held only one of them against the teeth, so as not to know the number of beats which ought to be produced. When this fork had ceased to be heard, we touched both forks to the wax in the ears, as before, and in every case the correct number of beats was at once heard. The experiment was varied slightly as follows: One ear only was closed with wax; the other was immersed in a large basin of water. The experiment was then repeated as above, with the difference that one fork, instead of being touched to the ear, was touched to the marble basin, its vibra-

tions being transmitted to the inclosed ear through the water. The same results were obtained as before. These experiments were all carried on at night when there was not the slightest disturbing vibration in the air. Together with another experiment described below, they lead us to conclude that aërial vibrations acting upon the ear are not transmitted through the skull or bony parts of the head from one ear to the other. The experiments cited certainly seem to bear out this view, at least for tones of feeble intensity. Furthermore, the fact that differential tones have not been produced binaurally leads us to believe that it is true generally, even for very intense sounds, that binaural beats do not result from sound conduction within the head.

In his experiments Thompson was never able to obtain differential tones unless the sounds were allowed to mingle before falling upon the ears. Our experiments fully bear out this result, which we have verified in the following ways: (1) By listening to Ut_4 and Mi_4 forks through the telephones. These notes were transmitted with more than sufficient intensity to give rise to a differential when heard in the ordinary manner; but with a telephone placed at each ear not the faintest trace of such a tone could be heard. We also reached equally negative results on listening to Mi_4 with a telephone, and holding an Ut_4, Sol_4, or Sol_3 fork close to the other ear, or on leading their vibrations to the ear through a long rubber tube. (2) By Thompson's method of carrying the tones to the ears by means of rubber tubes (in our experiments about thirty feet long), the forks being struck, one in another room and the other outside of the window, to prevent their tones from mixing before reaching the ears. No differential tone could be heard. We also tried to use Koenig's forks for illustrating beat-notes, but their very rapid vibrations were so deadened by the tubing that they could only be heard very faintly. (3) By the same process as above, with the exception that the ends of the rubber tube were connected with cylindrical resonators which were blown as already described. This gave almost deafening tones in each ear, but not the slightest trace of a differential. But in this case, as also in (2), if the two tubes were connected to two branches of a Y tube, and a third rubber tube leading to the ear was connected to the third branch, the differential tone came out at once. (4) The most striking proof was the following: The ears being closed with wax, a brass rod about five feet long was held lightly against the wax in each. When the stems of forks struck by two assistants were pressed against the farther end of the

rods, very loud tones were heard in the ears, unaccompanied by any differential tone. If, however, one of the rods was removed from the ear and pressed tightly against the head, or better, against the teeth, a loud differential tone was heard at once in the ear against which the rod was placed. If both rods were held against the teeth or head the differential tone was heard in both ears.

This apparent impossibility of producing differential tones when the primary tones are caused to actuate separate ears would seem conclusive of the point under discussion, except for the fact that it might reasonably be argued that if sound conduction through the skull occurred from one ear to the other, the vibrations thus conducted might act directly on the inner ear, and not indirectly through the membrana tympani and chain of bones, in which case no differential tones should arise, although beats could still take place in the manner assumed by Helmholtz. But our experiment with the brass rods, described above, shows that such tones are in fact readily produced when conduction to the ear takes place through the bones of the head. And if there were any material conduction of this kind, it certainly seems as if the very loud sounds used by us would have given evidence of it at least by the occasional production of differential tones when the sounds actuated separate ears.

The production of beats without the simultaneous action of the beating sounds upon the same ear seems, therefore, to be clearly shown from these facts: (1) Beats are produced by sounds falling upon the ears separately even when the greatest precautions are taken to prevent both from affecting the same ear. (2) Under no circumstances does it seem to be possible to produce differential tones by binaural combination, even when the separate tones are extremely loud. (3) In all cases in which the beating sounds have apparently acted separately upon the auditory apparatus of each ear, the singular phenomenon of encephalic localization of the beats has appeared, while this phenomenon has never been found present either when the two sounds acted simultaneously upon the same ear through the auditory canal, or when one of them was manifestly transmitted to the ear through the bones of the skull.

We are therefore led to the conclusion that the mechanism in which the interference causing beats takes place, and through the operations of which we consequently recognize the character of consonant and dissonant intervals, lies in part, at least, beyond the audi-

tory apparatus of the inner ear. It seems to us clear that notes may beat when they do not affect the same vibrating elements of the inner ear, and that we must look to some more profound changes within the sensorium itself for a complete explanation of the phenomena under consideration.

It may be possible that there are two distinct causes of beating; one, that suggested by Helmholtz, which wholly or principally obtains when the two notes act simultaneously upon the same ear, and the other due to some different and more obscure action which obtains in the case of binaural beats. In this case quite different laws as to consonance might hold. The exceeding harshness of dissonances in binaural audition lends some support to this view.

ROGERS LABORATORY OF PHYSICS,
May, 1891.

FLOOD DISCHARGE FROM SMALL WATERSHEDS.

By DWIGHT PORTER, Associate Professor.

As cities grow, as manufacturing develops, as cultivation of the soil extends, a demand constantly arises for the impounding of water in reservoirs, to furnish a supply for domestic use, or for power, or for the crops in arid regions, and the country rapidly becomes dotted with the artificial lakes and ponds thus created. Commonly they are at the head waters of the small streams, where the latter are most readily controlled. Along the valleys below may be thriving villages and populous towns, liable to be inundated, and perhaps swept away at any moment, except as the vast energy of the waters stored above is restrained by an earthen or masonry dam.

Above the South Fork dam, by the failure of which, on May 31, 1889, more than two thousand lives were lost at Johnstown and its vicinity, and property to the value of millions of dollars was destroyed, were stored about 20,000,000 cubic yards of water. For years the waters of the reservoir were held back by a well-built embankment, until at length their opportunity came, and deliberately, almost leisurely, yet relentlessly, they cut their way through. In their report upon the failure of this dam,[1] Messrs. Francis, Worthen, Becker, and Fteley, a committee of the American Society of Civil Engineers, make this significant remark: "There are today in existence many such dams which are not better, nor even as well, provided with wasting-channels than was the Conemaugh dam, and which would be destroyed if placed under similar conditions." The conditions there present were, briefly stated, a substantially constructed earthen dam, a full reservoir, an extraordinary rainfall, an insufficient waste-way.

It is hardly conceivable that an earthen reservoir embankment would fail by sliding or by overturning as a whole, although such action is entirely possible with a timber or a masonry dam. Rather is the earthen structure likely to be overcome through the insidious

[1] Transactions of American Society of Civil Engineers, 24, 457.

action of a small current of water, finding for itself a passage through or over the embankment, and then with increasing speed enlarging this channel by erosion, until the trickling stream becomes a resistless torrent.

The percolation of water through an embankment is guarded against by careful selection and compacting of the material of the structure; and further, in general, by building at the center an impervious wall. The overtopping of the dam by water is designed to be prevented by providing a sufficient waste-way to carry off surplus storm water without allowing the reservoir surface to rise dangerously near the crest of the dam. The waste-way may be either a weir, over whose edge the surplus water shall escape to a suitable channel below the dam, or it may be a gate or set of gates, discharging into a pipe, tunnel, or other outlet. The important question arises, How large a discharging capacity shall the waste-way receive? Since the quantity passing a given weir or orifice under a given head can be computed with sufficient accuracy, the problem reduces to finding the volume of water for which it is necessary to provide an outlet. In other words, assuming the reservoir full to the safety line, what will be the probable maximum rate of discharge into it from the tributary watershed?

Clearly, the rate of greatest discharge must be largely dependent upon the amount of precipitation in severe storms. The amount and distribution of rainfall are matters either of record within, or closely adjacent to, the watershed under consideration, or of inference from the records of other and more or less distant localities. From the accompanying memoranda, which are intended to show the precipitation in a few notable storms, probably among the severest on record in the respective localities, it will be noticed that on the Atlantic coast rainfalls of from 6 to 10 inches in from twenty to sixty hours in single storms have repeatedly been observed; and that in the South and on the Pacific coast, even those figures have been far exceeded:

Storm of October 3–4, 1869. General over eastern part of United States. Rainfall heaviest in Connecticut. Maximum recorded rainfall during storm 12.35 inches, at Canton, Connecticut. At various stations 8 or 10 inches of rain fell in from thirty to forty hours, and upon an estimated area of more than 24,000 square miles the total precipitation was 6 inches or more, occurring in from thirty to fifty or sixty hours.

JAMES B. FRANCIS,
Trans. Amer. Soc. Civ. Engs., 7, 224–235.

Storm of August 16–19, 1879. Rainfall recorded at Chestnut Hill Reservoir, Boston Water Works, 4.68 inches in ten hours; 6.23 inches in seventy-one hours.

DESMOND FITZ GERALD,
Trans. Amer. Soc. Civ. Engs., 21, 96, Plate XXIX.

Storm of February 10–14, 1886. Precipitation in this storm estimated to have exceeded 8 inches upon an area of 750 square miles. Rainfall at New London, Connecticut, 6.66 inches in twenty-four hours; 8.93 inches in fifty-eight and one-half hours.

WINSLOW UPTON, *Science*, 7, 256.

On Stony Brook watershed, near Boston, the rainfall in this storm is estimated to have been 5.86 inches in seventy-nine hours. (See report upon "Prevention of Floods in the Valley of Stony Brook," by J. B. Francis, E. C. Clarke, C. Herschel, Commissioners.)

On the Sudbury River watershed the observed rainfall in the same storm was 4.64 inches in seventy-seven hours.

DESMOND FITZ GERALD,
Trans. Amer. Soc. Civ. Engs., 25, 254.

On both these watersheds the storm was accompanied by the melting of snow and ice estimated as equivalent to an additional 2 inches of rainfall, and the discharge of the streams rose to an almost unprecedented amount.

Storm of May 30–31, 1889. Very severe over an area of some 20,000 square miles, lying mainly in Pennsylvania. Rainfall believed to have been 6 or 8 inches on South Fork watershed. At Grampian Hills, adjacent to South Fork basin, the observed rainfall was 8.60 inches in thirty-one hours. Near the summit of the Alleghany Mountains the rainfall is reported to have been from 8 to 10 inches in from eighteen to thirty-six hours. (See report of Committee on "Cause of Failure of South Fork Dam," *Trans. Amer. Soc. Civ. Engs.*, 24, 446, 447.)

Storm of July 29–30, 1878. Rainfall recorded at Washington, D.C., 5.80 inches in nineteen hours. *Records of United States Signal Service.*

August 5, 1843. At sundry points in Pennsylvania, rainfall reported at 13 inches in three hours, 16 inches in three hours, and 10 inches in two hours, respectively.

American Weather, by Gen. A. W. Greely.

September 29, 1882. At Mayport, Florida, 13.7 inches of rainfall in twenty-four hours.

American Weather.

September 21–23, 1886. At Brownsville, Texas, rainfall of 23.14 inches in sixty-four and one-half hours. *American Weather.*

Recorded in 1884, at St. Louis. Rainfall of 5.05 inches in one and one-fourth hours; 7.55 inches in twenty-nine hours.

E. KUICHLING,
Trans. Amer. Soc. Civ. Engs., 20, 12.

February 21–22, 1891. Rainfall recorded at Bear Valley reservoir, California, 17 inches in twenty-four hours; 19 inches in twenty-six hours. At Cuyamaca dam, forty miles east of San Diego, the rainfall was 23.4 inches in fifty-four hours, of which 13 inches fell in thirteen hours, and 7 inches in ten hours. *Engineering News*, March 28 and April 18, 1891.

The rate of flow caused by a storm further depends upon:

(*a*) Duration of storm.
(*b*) Extent of watershed.

(*c*) Sundry physical features of watershed, such as steepness or imperviousness of surface.

(*d*) Sundry accidental conditions, such as saturation of the ground; presence of a body of snow; or a close succession of storms.

Shortly after the beginning of a storm the flow at the outlet of the watershed will be representative of the rainfall upon the immediately adjacent area only. The area thus participating will increase with the duration of the storm; but the entire area geographically tributary at the outlet will be effectively so only after a certain time, varying with each watershed. It may require six hours for rain from some parts of the periphery to reach the outlet, and if the storm last but one hour it is plain that the entire area of the drainage basin will not be represented in the flow at the latter point, which may be the waste-weir of the reservoir. Phenomenally high rates of precipitation during storms generally continue for but a short time. Thus at Washington, D.C., in the period 1871–1887, fourteen storms were recorded, which furnished one inch or more of rain in one hour; but although in certain storms the *rate* of precipitation rose to 6 inches, 8 inches, and even nearly to 10 inches per hour, such intensities were of short duration. This is shown by the fact that in no one of the storms did the actual depth of rain falling in a continuous period of one hour exceed 1.8 inches. The highest *rate* was reached in a shower lasting but six minutes, a time far too brief for concentration of the discharge from a natural surface of any considerable size.

In spite of the high rate of precipitation often attained in brief showers, it will in fact be found that the maximum rate of flow from most natural watersheds results, not from such downpourings, but from prolonged heavy storms of one or two days' duration, in which, very likely, no remarkable intensity of rainfall is at any time reached. It will therefore generally be of more value to consider for how much rain falling in a period of say six, or twelve, or twenty-four hours, provision should be made, than to consider the high intensities sometimes recorded for a fraction of an hour. A consideration of the latter is more profitable in connection with problems of sewer discharge, which is quick to respond to rainfall upon the impervious surfaces of cities.

Even if the rainfall were uniform in intensity over the entire drainage basin, and from beginning to end of the storm, the size of the basin relatively to the duration of the rain would be important, as has

been pointed out, in respect to concentration of discharge at the outlet. The absolute size of the drainage basin may be of further importance, since in fact the intensity of rainfall is not uniform over the whole area, the maximum often being confined to a comparatively small portion. Thus, if the given watershed were of large size, it might be raining heavily upon one part and lightly or not at all upon the rest; in which case the resulting discharge at the outlet would be moderate in rate, as averaged for the entire area. The chances are, therefore, general physical features continuing the same, that the rate of maximum discharge will increase as the size of drainage area diminishes. The larger rivers of New England, with watersheds of several thousands of square miles each, the Connecticut and the Merrimack, for example, show maximum rates of discharge not exceeding 20 or 25 cubic feet per second per square mile of basin; while it is tolerably easy to find examples of streams in the same region draining not more than one hundred square miles each, whose flood discharge rises to 40, 50, or even a larger number of cubic feet per second per square mile.

The following memorandum, from a paper by Mr. James B. Francis,[1] shows the estimated areas over which specified depths of rain fell during the remarkable storm which visited New England, October 3 and 4, 1869:

6	inches	or	more,	24,431	square miles.
7	"	"		9,602	" "
8	"	"		1,824	" "
9	"	"		1,046	" "
10	"	"		519	" "
11	"	"		179	" "

A similar estimate, as below, has been made by Prof. Winslow Upton,[2] as to the distribution of rainfall in the great storm of February 10–14, 1886, which was most severe over Rhode Island:

Between	4	and	5	inches,	2,750	square miles	(land surface).
"	5	"	6	"	1,850	" "	"
"	6	"	7	"	1,500	" "	"
"	7	"	8	"	750	" "	"
	Over		8	"	750	" "	"

[1] Transactions of American Society of Civil Engineers, 7, 234.

[2] Science, 7, 254.

Mr. Emil Kuichling estimates from personal observation that for brief showers, lasting say a quarter of an hour, the least area covered by the heaviest precipitation is probably half a dozen square miles.[1] From such figures as have been given it appears that substantially the maximum intensity of rainfall in a storm is likely to prevail over at least as large an area as is commonly tributary to a storage reservoir.

It is further evident that the maximum rate of flow will be largely influenced by physical characteristics of the watershed. Steep slopes, by shedding quickly the rain to the water-courses, not only limit its chance to percolate into the ground, but have also the same influence as increased duration of storm, in rendering all parts of the drainage area effectively tributary at the outlet. Imperviousness of surface, whether due to rock exposures, as in the mountains; or to the hammering of the soil by the hoofs of animals, as on the Western prairies; or to other conditions, has a marked effect upon discharge. This can readily be appreciated by comparing the behavior of a mountain torrent (although in that case also influenced by steepness of slopes), or that of a Colorado prairie stream, with that of a stream draining the sand hills of the South Atlantic or Gulf coast. The presence of woods, swamps, lakes, ponds, or artificial reservoirs, all of which tend to hold back the rain delivered to them, acts powerfully to equalize the flow of streams and to modify flood discharge.

The various features that have been mentioned all have a bearing upon maximum discharge; but their influence is not more important than that of certain accidental conditions other than the rainfall itself, such as the degree of saturation of the soil, or the presence of a large volume of melting snow at the time of a storm, for example. It is the favorable combination of accidental with fixed and permanent conditions that seems likely to cause remarkable floods. The heaviest rainfall does not necessarily, of itself, produce the highest discharge, as is strikingly shown below in the case of the West Branch of the Croton River. (J. James R. Croes, Transactions of American Society of Civil Engineers, 3, 78.)

[1] Transactions of American Society of Civil Engineers, 20, 13.

DISCHARGE OF WEST BRANCH OF CROTON RIVER.

Date of Storm.	Rainfall Inches.	Accompanying greatest discharge of stream. Cubic feet per second.	
Oct. 3–5, 1869.	5.23 in 60 hours.	350	Storm occurred after protracted drought.
Aug. 2, 1867.	1.96 in 17 hours.	93	August 1, before the storm, the discharge was only about 8 cubic feet per second.
Aug. 16, 1867.	3.38 in 15 hours.	676	Frequent rains, some of them heavy, in previous two weeks.
Feb. 18, 1870.	2.41 in 28 hours.	970	Storm accompanied by rise of temperature and rapid melting of snow. The actual maximum discharge was not measured, but the figures given are Mr. Croes' estimate.

If rain were to fall for a considerable length of time upon an absolutely impervious surface, the resulting discharge from the area would be substantially the same in rate as the precipitation. Indeed, if the surface were first covered with snow, a warm rain might result in a discharge at a rate much greater than that of the precipitation. In fact, in the northern part of the United States the discharge from a given watershed for an entire spring month is often much greater than the entire precipitation for the same time. Thus, for March, 1877, although the rainfall upon the basin was abnormally large, the discharge of the Sudbury River for the month was three per cent. in excess of the rainfall; and there is seldom a year in which, for some of the months, the discharge of this river does not much exceed the contemporary precipitation, rising in some cases to between two and three times the latter.[1] These figures deal, however, with months, and not with the periods of single storms; and the writer is not aware that in any recorded instance the rate of flow from a natural drainage area of any considerable size has risen above, or even nearly as high as, the maximum rate of rainfall for the locality.

[1] *The Flow of the Sudbury River.* Transactions of American Society of Civil Engineers, 10, 232–234.

Various influences have here been presented as affecting maximum flow; yet to assign to them definite values, which shall reduce the whole question to a simple problem, admitting of exact solution, is not practicable with the meager data available as to the actual flow of streams and the attending conditions. It has been seen that in a general way the maximum flow is dependent upon rainfall and upon drainage area; but the details of the relationship are so disguised and so variable that it cannot be nicely traced. In estimating the probable maximum discharge for a given case, the engineer should determine the area of watershed. He may then assume a flow, in inches on watershed, or in cubic feet per second per square mile, founding his assumption upon the maximum known yield of other streams. The principles which have been brought forward should find their application in suiting the assumption to the peculiar conditions of the case in hand. The provision for storm water should be a generous one, for upon it may depend the very existence of the dam and reservoir, while the outlay involved in a suitable waste-way is likely to be, at the most, small relatively to the cost of the works as a whole.

So far as recorded measurements of flow are to be found, there is little to indicate for any of the streams of the North Atlantic slope, and for such drainage areas as we are considering, a greater rate of flood discharge than 6 or 8 inches per twenty-four hours upon the tributary watershed (160 to 215 cubic feet per second per square mile). The Sudbury River, draining what is described as a "fair average New England watershed," some seventy-five square miles in extent, has shown a maximum discharge at the rate of about 1.65 inches per twenty-four hours from the watershed (about 44 cubic feet per second per square mile). In the construction of reservoirs in the basin of this river, for the water supply of Boston, waste-ways are proportioned to discharge the equivalent of 6 inches per twenty-four hours from the tributary watershed, a provision which certainly appears liberal. The same assumption, of 6 inches per twenty-four hours, is stated by Mr. Alphonse Fteley, Chief Engineer of the Croton Aqueduct Commission, to be uniformly employed by him in providing for maximum discharge from watersheds of not more than one hundred square miles, the rate being modified for larger areas.

Through the courtesy of Mr. Desmond Fitz Gerald, in charge of the Western Division of the Boston Water Works, the writer is permitted to reprint from the "Transactions of the American Society

of Civil Engineers" for September, 1891, the accompanying diagram, which shows in a most interesting way the history of the flow of the Sudbury River, and its affluent, Cold Spring Brook, during the storm of February 10–13, 1886, in which the discharge was at the highest rate ever observed for those streams. The diagram will in the main explain itself. It will be noticed that ordinates to the curves show cumulative rainfall and discharge from the beginning of the storm, while the slope of either curve at any point indicates the rate of precipitation or of discharge, as the case may be. Data of the character and extent of those which have been collected for the Sudbury River are almost without parallel, and for them every civil engineer and every community concerned with hydraulic works is under obligation.

In the table that follows are presented data, collected from a variety of sources, showing the observed flood discharge of sundry streams, mainly with small tributary drainage areas. Doubtless in many of these cases the discharge was not determined with a high degree of accuracy, and it may be that more extended observations would have shown for some of the streams higher figures than are here given. Nevertheless, the results are the best that are at command, and are believed to be sufficiently accurate to make them valuable for reference.

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Stream.	Locality.	Drainage area. Sq. miles.	Flood discharge. Cu. feet per sec.	Equivalent in cubic feet per sec. per sq. mile.	Equivalent in inches per twenty-four hours on watershed.	Remarks.
Cold Spring Brook .	Massachusetts . . .	6.43	312	48.4	1.80	February, 1886. Tributary of Sudbury River. D. Fitz Gerald, *Trans. Amer. Soc. Civ. Engs.*, 25, 257.
South Branch . . .	New York	7.8	577	73.9	2.75	1869. N. Y. State Counsel.*
Woodhull Reservoir	New York	9.4	731	77.8	2.89	1869. N. Y. State Counsel.*
Woodhead Reservoir	Manchester Water-Works, England .	11. ±	3,520 ±	320	11.9	J. F. Bateman.* *Minutes of Proceedings, Institution of Civ. Engs.*, 22, 362.
W. Branch, Croton .	New York	20.37	1,109	54.4 †	2.02	January 7, 1874. J. J. R. Croes.
Mill River	Massachusetts . . .	58	899 **	15.5	0.58	
Flat River	Rhode Island . . .	61	7,375	120.9	4.50	J. H. Shedd.* Freshet of March, 1843. Due to rain and melted snow.
Sudbury River . .	Massachusetts . . .	74.66	3,305	44.3	1.65	February 13, 1886. Average New England watershed, 38% wooded. D. Fitz Gerald. *Trans. Amer. Soc. Civ. Engs.*, 25, 257.
Park River	Connecticut . . .	76	2,378 **	30.4	1.13	Drains an open and moderately hilly district. No important reservoirs.
Rock Creek . . .	Georgetown, D.C. .	77.5	9,790 ††	126.3	4.70	W. R. Hutton.
Hockanum River .	Connecticut . . .	79	6,167 **	78.1	2.91	Important reservoir in upper waters.
Nashua River . . .	Massachusetts . . .	109	11,400	104.6	3.89	E. Sawyer.* Valleys narrow, with steep slopes. Two important reservoirs and numerous small mill ponds.

RECORDS OF FLOOD DISCHARGE OF STREAMS — *continued.*

Stream.	Locality.	Drainage area. Sq. miles.	Flood discharge. Cu. feet per sec.	Equivalent in cubic feet per sec. per sq. mile.	Equivalent in inches per twenty-four hours on watershed.	Remarks.
Assabet River . . .	Massachusetts . . .	115	2,184	19.0	0.71	1865. E. Sawyer.*
Scantic River . . .	Connecticut . . .	118	6,116 **	51.8	1.93	Watershed open and moderately hilly.
Pawtuxet River . .	Rhode Island . . .	190	10,800	56.9	2.12	1867. J. H. Shedd.*
Fish Creek	Saratoga Co., N.Y. .	236	1,964	8.3	0.31	1872. E. Sawyer.*
Croton River . . .	New York	338.82	25,367	74.9 †	2.79	April 20, 1854. J. J. R. Croes.
Westfield River . .	Salmon Falls, Mass. .	350	53,000	151.4	5.63	December 10, 1878. Discharge as estimated by Hiram F. Mills. See his report to town of Westfield. Freshet due to a storm in which the rainfall was some three inches, accompanied by the melting of six or eight inches of snow already on the ground. Watershed hilly, mountainous in part, well wooded, but not largely supplied with lakes or reservoirs.
Concord River . .	Lowell, Mass. . . .	352	4,450	12.6 †	0.47	March 30, 1876. Maximum of gaugings for three years only. C. Herschel.
Blackstone River .	Rhode Island . . .	480	4,250	8.9	0.33	1871. E. Sawyer.*
Farmington River .	Connecticut . . .	584	24,375 **	41.7	1.55	Watershed hilly and well wooded. Several important reservoirs.
Chicopee River . .	Massachusetts . .	706	9,635 **	13.7	0.51	Watershed in part hilly, with steep, wooded slopes; and in considerable part also open, moderately undulating, and with extensive marshes.

Potomac River . .	Cumberland, Md. . .	920	17,900 ††	19.5	0.72	November, 1877. Discharge approximate. W. R. Hutton. Watershed wooded, with narrow valleys and steep slopes.
Passaic River . . .	Dundee, N.J. . . .	981	19,944	20.23 †	0.76	December 12, 1878.
Merrimack River .	Lawrence, Mass. . .	4,136	96,780	23.40 †	0.87	1852. C. Herschel. Watershed wooded, and contains lakes and artificial reservoirs.
Delaware River . .	Lambertville, N.J. .	6,500 ±	350,000 ±	53.8	2.00	Ashbel Welsh. *Trans. Amer. Soc. Civ. Engs.*, 10, 241. Watershed hilly and rolling, well wooded, many lakes.
Connecticut River .	Holyoke, Mass. . .	8,006	168,918	21.1	0.79	Watershed hilly, and in part mountainous, well wooded, and contains many lakes and artificial reservoirs. Discharge given for freshet of May, 1854. T. G. Ellis, *Report of Chief of Engs.*, U. S. A., 1878, Appendix B, 14, p. 108.
Kanawha River . .	Charleston Pool, Va.	8,900	118,291 ††	13.2	0.49	August 3, 1875. Scott. Watershed mountainous and wooded. No lakes.
Potomac River . .	Georgetown, D.C. .	11,124	175,000 ††	15.7	0.59	Spring of 1852. Discharge approximate. W. R. Hutton.
Mississippi River .	Columbus, Ky. . .	930,540	1,475,000	1.59	0.059	June 16–22, 1858. Humphreys and Abbot. *Report on the Mississippi River*, p. 181.

* Transactions American Society of Civil Engineers, 4, 301.

** Assumed maximum discharge. T. G. Ellis, Report of Chief of Engineers, U. S. A., 1878, Appendix B, 14, p. 55.

† Report of J. J. R. Croes and Geo. W. Howell to Newark Aqueduct Board, 1879.

†† Transactions American Society of Civil Engineers, 10, 242.

SOME RESULTS OF SANITARY LEGISLATION IN ENGLAND SINCE 1875.[1]

By GARY N. CALKINS, S.B.

THE *Revue Scientifique* for April, 1891, contains a review of an article by M. H. Monod on the results of sanitary legislation in England. This article is entitled *Les résultats des mesures sanitaires en Angleterre depuis 1875*, and advocates the extension of sanitary measures in France, where the diminution in population has lately caused much concern.

In 1875 a general law was passed in England for the protection of the public health. This was known as the Public Health Act, and since this Act was passed the death rate in England has decreased for all diseases which owe their origin and growth to defective drainage and impure water supplies. Typhoid fever is such a disease, and the diminution of 57 per cent. in the death rate from this malady is undoubtedly the greatest triumph for sanitary reformers.

The cost of sanitary improvements up to 1890 was about $583,500,000, or a mean annual expense of about $42,000,000, and the immediate effect was a marked decrease in the number of deaths, as the curve indicates.

During the ten years from 1866 to 1875 the average annual mortality was 22.19 per 1,000 inhabitants living, as the following figures show:

Year.	Rate.	Year.	Rate.
1866	23.4	1871	22.6
1867	21.7	1872	21.3
1868	21.8	1873	21.0
1869	22 3	1874	22.2
1870	22.9	1875	22.7

[1] Republished, with additions, from the Quarterly Publication of the American Statistical Association, 2, 297.

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From 1838, the first year that registration was taken in a careful manner, to 1865, the average annual rate was about 22.35 per 1,000 living. It is sufficiently accurate to state that, for the period 1838 to 1875, the average death rate remained about the same.

But for the ten years of the period 1880 to 1889 the average falls to 19.08; and it is interesting to note that the curve representing the rate since 1875 does not show the great fluctuations which are characteristic of the period before that time. This is probably due to the few epidemics in the last fifteen years, that of 1878 being the most noticeable, when there was a large infant mortality from diarrhœa and whooping cough. The great rise in 1849 and again in 1854 was caused largely by an epidemic of cholera. In 1863, 1864, 1865, and 1866, typhoid fever kept the death rate up to an average of 24.31 per 1,000. Since 1878 the rate is almost uniformly descending, as the following figures show:

Year.	Rate.	Year.	Rate.
1879	20.7	1885	19.0
1880	20.5	1886	19.3
1881	18.9	1887	18.8
1882	19.6	1888	17.8
1883	19.5	1889	17.9
1884	19.5	. . .	. . .

It seems justifiable to assign as the cause of this diminution in the death rate the operation of the Public Health Act and the execution of duties such as drainage, inspection of water supplies, vaccination, and others, which are becoming better and better understood.

Mr. Farr, in his *Vital Statistics*, estimates the value of human life in England to be about $770 a head. "The minimum value of the United Kingdom, men, women, and children, is £159 ($770.36) a head; that is the value inherent in them as a productive, money-earning race." (W. Farr, *Vital Statistics*, page 61.)

The economic value to England of improvements which will check certain diseases, such as typhoid fever, is obvious; for this complaint is especially virulent to man in the adult age, that is, in the age when he is producing to the best of his ability. From the registration sta-

tistics we can calculate the value of the expense to England of the vast sums which she has given out for the protection of the public health.

If we suppose, which is allowable if other things remain the same, that the mortality for each year of the period 1880 to 1889 had been the same as the average annual death rate for the period 1866 to 1875, then, comparing this average with the true rate, we find the number of lives saved has been:[1]

Year.	Lives Saved.	Year.	Lives Saved.
1880	43,457	1885	87,722
1881	85,743	1886	80,546
1882	68,412	1887	95,758
1883	72,013	1888	125,680
1884	72,996	1889	124,477

The total gives a gain of 856,804 lives saved, and this, according to Mr. Farr's estimate, represents a social capital of more than $650,000,000. Thus in ten years the country has more than regained the sum that was spent for sanitary improvements in the fifteen years; and in this calculation nothing figures for maladies avoided; nothing for that which cannot be expressed by figures — spared grief, better health, and happier life.

Mr. Noel A. Humphreys, in an article published in the *Journal of the Statistical Society*, has proved that, if the death rate should continue as low as it was from 1876 to 1880, the duration of the average life of men would be prolonged two years; that of women more than three years as compared with the English life tables; and that 70 per cent. of men and 65 per cent. of the women concerned would be from twenty to sixty years of age, or, in other words, that the extra life would be added to the most productive age. (Longstaff, *Studies in Statistics*, page 226.)

The diminution of mortality is not observed in all forms of disease, and the relative importance of sanitary measures for certain diseases is shown in the following table. The mortality from zymotic diseases

[1] I have found mistakes in the *Revue d'hygiène* in the calculation of these figures, and have therefore re-computed them from the Report of the Registrar General for 1889.

from 1861 to 1870 was 42.54 per 10,000 living, and this was reduced to 24.52 in the period from 1880 to 1889, thus diminishing 18.02 per 10,000 inhabitants. This diminution was distributed in the following order:

Measles	0.02	Diarrhœa, dysentery	2.56
Diphtheria	0.33	Scarlet fever	5.92
Whooping cough	0.78	Typhoid fever	6.36
Cholera	0.91		
Small pox	1.14	Total	18.02

Thus, while the number of deaths from typhoid fever and scarlet fever has decreased notably, measles, diphtheria, and whooping cough have apparently almost completely escaped the influence of sanitary measures.

Consumption has equally diminished in England in these last years. The mortality from this cause was, in the years 1861 to 1870, 24.89 per 10,000 living. For the period 1880 to 1889 it fell to 17.36, a difference of 7.53 per 10,000.

Let us see in just what manner England has gained by these sanitary improvements; at what period or age life has been saved, and whether the average length of life has been affected. The reports of the Registrar General furnish the necessary data.

In studying the figures for the decade 1871 to 1880 it will be seen that the death rate is unequally distributed for the period of the earlier years, but rises for the later years, for men after thirty-five, for women after forty-five. This is shown by an instructive table taken from Longstaff's *Studies in Statistics*, and printed on pages 333 and 334. This shows the death rates for males and females from all causes per 1,000 living at various groups of ages. The average death rate for fifty years is printed in heavy type, as are also the figures for the decade in which the maximum death rate at each age was reached. The last row of figures indicates the rise or fall per cent. of the average death rate for the last decade above or below the average for fifty years.

From this table we see that for the early periods of life the death rate has decreased. In the first decennium, for example, the maximum rate for males between the ages of five and ten was 9.23 per 1,000 in the period 1839–48. The average for the fifty years is 7.78, and the fall from this average in the decade 1879 to 1888 was 28.5 per cent.

For the years between ten and fifteen the death rate decreased from the maximum 5.11 in the period 1849–58, to 3.08, and the fall from the average 4.34 for fifty years was 29 per cent. For the years between fifteen and twenty the fall was 26.3 per cent. from the average 6.04. But for the years between thirty-five and forty-five the maximum death rate was, in the decade 1869–78, 13.97, and in the last decade 12.51, and the diminution from the average 13.03 for the fifty years falls only 4 per cent.; while for the years forty-five to fifty-five the average rate of the last decade *exceeds* the average for the five decennia by 2.6 per cent., for the years fifty-five to sixty-five by 3.3 per cent., and for the years sixty-five to seventy-five it exceeds the average by 3.7 per cent. But above the age of eighty-five years the death rate diminishes from the average by 1.1 per cent. The female mortality is about the same as that for males. It will be observed, however, that the rise per cent. in mortality during the last decade for males, at the age of from forty-five to fifty-five, is 2.6 above the average for fifty years, whereas for females of corresponding age there is a fall in mortality of 2.6 per cent.

DEATHS PER 1,000 LIVING. MALES.[1]

Periods.	All Ages.	0–5	5–10	10–15	15–20	20–25	25–35	35–45	45–55	55–65	65–75	75–85	85–
1839–1848	23.05	71.76	**9.28**	5.06	**7.09**	**9.45**	9.80	12.65	17.83	31.43	66.89	147.5	313.1
1849–1858	23.27	72.68	8 86	**5.11**	6.87	9.10	**9.86**	12.80	18.38	31.35	65.88	146.8	306.2
1859–1868	**23.47**	**72.92**	8.07	4.50	6.28	8.55	9.75	13.22	18.90	32.62	66.40	146.3	313.4
1869–1878	23.13	69.96	7.18	3.93	5.53	7.75	9.74	**13.97**	**20.01**	**34.53**	69.29	**149.5**	**322.7**
1879–1888	20 48	60.13	5.56	3.08	4.45	5.88	7.96	12.51	19.40	33.84	**70.24**	147.7	309.5
50 Years.	***22.68***	***69.49***	***7.78***	***4.34***	***6.04***	***8.15***	***9.42***	***13.03***	***18.90***	***32.75***	***67.74***	***147.6***	***313.0***
Fall or rise per cent.	−9.7	−13.5	−28.5	−29.0	−26.3	−27.9	−15.5	−4.0	+2.6	+3.3	+3.7	+0.1	−1.1

[1] Longstaff, *Studies in Statistics*, page 269.

DEATHS PER 1,000 LIVING. FEMALES.[1]

Periods.	All Ages.	0-5	5-10	10-15	15-20	20-25	25-35	35-45	45-55	55-65	65-75	75-85	85-
1839-1848	21.45	61.52	**8.96**	**5.46**	**7.94**	**8.96**	10.29	12 58	15.63	27.94	60 C2	**185.1**	**293.3**
1849-1858	**21.66**	63.04	8.71	5.25	7.64	8.87	**10.35**	**12.68**	**15.80**	27.74	59.29	134.5	286.1
1859-1868	21.27	**63.20**	7.83	4.60	6.79	8.11	9.78	12 08	15.51	27.65	58.95	134.0	286.7
1869-1878	20.47	59.91	6.68	3.94	5.77	7.14	8.94	11 79	15.71	**28.49**	60.50	135.0	292.8
1879-1888	18 30	50.83	5.40	3.22	4.62	5.76	7.63	10.67	15.16	28.07	**61.12**	132.8	280.3
50 years.	***20.63***	***59.70***	***7.52***	***4.49***	***6.55***	***7.77***	***9.40***	***11.96***	***15.56***	***27.98***	***59.98***	***134.3***	***287.8***
Fall or rise per cent.	—11.3	—14.9	—28.2	—28.3	—29.5	—25.9	—18 8	—10.8	—2.6	+0.3	+1.9	—1.1	—2.6

[1] Longstaff, *Studies in Statistics*, page 269.

From this table we see that sanitary measures affect the death rate for persons between the ages of one and twenty-five years, and especially during the period of youth from ten to twenty years. Economically this is a great gain; for the 28 per cent. of those who might have died during this period have now tided over the age of non-production, when they were a burden on the state, into a producing age. Had they died at the age of twenty or thereabouts, the country would have gained no recompense for the expense of maintaining them through the non-productive period.

Or, to put this another way.[1] In 1854 a new-born boy might hope to live 39.91 years. But his son, born in 1880, could hope for 41.35 years. Now suppose both attain to the age of forty. In the first case 26.06 more years of life might be expected, but in the second case only 25.30. Again, suppose 1,000,000 male children were born during the period 1838–54, then at the age of fifty-five 409,460 would survive; of 1,000,000 male children born during the period 1871–80, at the same age of fifty-five, 424,677 would survive. But, at the age of seventy-five, 148,076 would survive in the first case, while in the second there would be only 144,960. That is, the death rate among persons of mature age is greater than formerly. This may be due to the fact that, with improved sanitary precautions, more feeble children live to maturity and die before attaining to old age; or perhaps because of the bustle of the nineteenth century, of the wear and tear upon the nervous system due to competition in densely packed cities and towns.

The effect of sanitary improvements is most noticeable upon infant mortality. If we take 858,878 as the mean annual number of births between 1871 and 1880, and compare it with the average annual number of births for the period between 1838 and 1854, we find the difference between the new life tables and the old to be an annual gain of 1,800,047 years,[2] and this alone is a great defense of the sanitary work now going on. How this may tend to weaken the race is not for me to surmise; at all events it is too late to return to Spartan methods of maintaining it perfect.

France, it is affirmed, is sadly in need of such sanitary improvements as have diminished the death rate in England during the last twenty years. At present her population is at a stand-still; but it

[1] Edinburgh Review, January, 1891, Sanitary Progress.

[2] The above figures are taken from Sanitary Progress.

should increase, and unless it does, there is something wrong in the body politic, since all surrounding countries are growing rapidly in population. Whatever the cause may be for such a small number of births per year, whether moral, or physical, or political, they are all remote and obscure, but the death rate is something that can be viewed and compared, and its causes investigated. Dr. Brouardel,[1] in a recent address before the *Académie de Médecine*, called attention to the disproportionate death rate in France from small pox and typhoid fever. In Germany 110 persons are lost annually by small pox, while in France the number is no less than 14,000. Dr. Brouardel attributes this to the rigidity with which vaccination is enforced in Germany, and the laxity in France.

There are 23,000 deaths from typhoid fever per year in France, and this was shown to be largely due to water supply. Thus in Vienna, before pure water was introduced, the death rate was 200 per 100,000 inhabitants; after the introduction of good water it fell to 10.

In Angôulème, after the introduction of pure water, the death rate from typhoid fever was reduced in the proportion of 18 to 0.063. In Amiens, after pure water was introduced, the number of deaths fell from 111 in 10,000 to 7; in Rennes from 43 in 10,000 to 2. Dr. Brouardel affirms that if vaccination and re-vaccination were obligatory in France, and that if towns were compelled to supply pure water, the saving of life would amount to from 25,000 to 30,000 annually, and this would make considerable difference in the population.

[1] London Lancet, January, 1890.

THREE CRITICS: MR. HOWELLS, MR. MOORE, AND MR. WILDE.[1]

By G. R. CARPENTER, Associate Professor of English.

Of the new books in English that have fallen into my hands during 1891, the three that have interested me most are Mr. W. D. Howells's "Criticism and Fiction," Mr. Moore's "Impressions," and Mr. Wilde's "Intentions."[2] They bear a strong resemblance to one another. They are alike in being collections of short pieces, published from time to time in current periodicals, and expressing opinions with which the ordinary reader has become, by hearsay or by actual knowledge, somewhat familiar. They are alike in being, not poems or novels or plays — not, in short, the kind of work usually styled creative — but critical essays, parasitical, as distinguished from strictly original work. They are alike, too, in that popular opinion allows a certain degree of opprobrium to rest upon their authors. The newspapers jest, properly enough, on Mr. Howells's extreme proclivity for realism; occasionally they even — improperly — sneer at it. In America, at least, Mr. Wilde is as often referred to unceremoniously by his first name as his last, and the impression of the majority is to the effect that intellectually he deserves scarcely more respect than the most empty-headed of dandies. Mr. Moore's name is not so familiar to us, but those who know it at all are apt to associate it with somewhat scandalous novels, or with articles that trumpet the fame of some hitherto unknown author, or with random items in the papers to the effect that, in the columns of So-and-So, Mr. Moore, the brilliant young novelist, has made a violent attack on This-and-That. But

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[1] Reprinted by permission from the *Andover Review* for December, 1891.

[2] Criticism and Fiction, by W. D. Howells, Harper & Brothers, 1891. Impressions and Opinions, by George Moore, Charles Scribner's Sons, 1891. Intentions, by Oscar Wilde, Dodd, Mead & Company, 1891.

in spite of whatever prejudicial associations of this sort one may quite casually and innocently have acquired concerning these three writers, the candid reader will find in their books much that is pleasant and instructive. More than that: from the work of men so totally different in their essential characteristics, he may obtain a singularly complete idea of the most important aspects of modern criticism. But this implies a rough analysis of the three books before us.

To judge "Impressions" fairly, one must first frankly rid himself of some surface irritation. Mr. Moore's inveterate habit of throwing all possible emphasis on the pronoun of the first person, his frequent swagger, his reckless grammar, and his indecorous heedlessness in many matters that deserve accuracy, are all displeasing. It is hard to keep one's temper when he insists on telling us what he said to Turgueneff, and not what Turgueneff said to him; when he habitually prefixes his "I told you so" to each account of a dramatic success or failure; or when he asserts complacently his ignorance of facts about which the nearest book of reference would inform him. Such faults we easily pardon in one writing in headlong haste, to meet an immediate exigency. They are not so easily passed over in a volume of collected essays, in which, more than in books of any other sort, it is safe to assume that the writer works with entire leisure, and with the advantage of adequate previous criticism. Unessential faults, however, the earnest reader can bear with. Why should such trifles of detail spoil his real pleasure? For in books, as in life, minor faults are often the inseparable concomitants of major virtues. The honest man may be blunt even to rudeness, or the sensitive man unbearably fastidious. With defects that spring from virtue these frailties of Mr. Moore's, as we shall see, may be charitably classed.

What strikes first the reader of Mr. Moore's criticism is its refreshing provinciality. It is not the parochialism of a small city or a small clique, but the provincialism of Paris and a part of London. In this he betrays his foreign life and training, and illustrates the trend of his genius. His criticism observes strict historical and geographical limits; it is not concerned with Greek art or the Roman decadence, nor does it make capital of those continued references to the Renaissance, or the fundamental unity of the arts, which it is now the fashion, following Mr. Pater, to introduce with a specious languor into the criticism of any subject. It is not concerned with that scholar's will-o'-the-wisp, comparative literature; not, in short, with aught but what

passes in Paris or in the Anglo-French circles in London for current coin. To the wider if vaguer movements of contemporary literature, Mr. Moore turns a deaf ear. He can write ardently about the Théâtre Libre in Paris without a single hint of her younger and healthier sister at Berlin — healthier, in that around her seem to cling more noble and more human ambitions. Russia, indeed, he knows through the novels of Tolstoi and Turgueneff, and whatever else has been reproduced in French. Ibsen, in spite of his enthusiasm, he quite fails to understand, treating his characters as if they were French, and forgetting that in not one of Ibsen's plays could the action be imagined as going on outside of Scandinavian or Anglo-Saxon surroundings. To the current movements in America, in Italy, in Spain, and in Germany, he never so much as alludes. Germany, indeed, is to him a land still barbarous, and in literature capable only of "Faust," lexicons, and fairy stories.

We have in Mr. Moore's criticism, then, provinciality pure and simple, and a literary interest geographically as limited as if it reached only from Boston to Portland. But it is just this circumscription of interest that, particularly in the Latin races, has been productive of such splendid results in literature, and Mr. Moore is so thoroughly French in his style and his sympathies that one would be puzzled to find the Anglo-Saxon in him. He has nothing of the Englishman's reserve, nothing of his strict holding to models and ideals, of his grim, deductive following out of alleged established truth. In its place we find that supple and vivacious curiosity, that never resting desire to see or hear some new thing, which dignifies and vitalizes French interest in all artistic matters, and, behind this real desire to be amused in whatever ways art will amuse one, a deeper feeling that literature is often (not always) a serious instrument for the propagation of vital truths. Mr. Moore's work, indeed, is not unlike that which we associate with the "Figaro." His style has all the "Figaro's" pert jauntiness, its abruptness and seeming incoherence, and, with them, its admirable sense of just how far one can go in the development of any idea; he has its frankness of expression, its familiarity, its odd Latin way of wearing its heart on its sleeve. This single quality of individuality is Mr. Moore's redeeming virtue. I cannot discover that he has any theory about literature, except that what interests and moves him is, *ipso facto*, interesting and moving. Such liberality of taste, even within the limits we have pointed out, is not a common quality;

even when found, it is too often evanescent. But while it lasts — this healthy equipoise of taste and appetite — it is worthy of sincere admiration. We in America especially, who are in literature by far too much inclined to feed on the prehistoric or the exotic rather than on what is, so to speak, the daily bread of literature, may gain a zest from Mr. Moore's example. His taste is discriminating but heartily sympathetic, and, as a rule, carries the reader with it. Naturally he thinks Balzac the novelist one should read first and cling to longest, but he nevertheless appreciates Turgueneff and Zola, while in Verlaine he finds a singular note of distinction. Nowhere is he persistently immoderate; everywhere he gives natural expression to his distinct individuality — an individuality unpedantic and as yet unsurrendered to the guidance of any system of æsthetics save that which is instinctive in him.

Sharply distinct from that individual criticism which, good or bad, forms the basis of Mr. Moore's "Impressions," is the theoretical criticism which we find demonstrated and illustrated in Mr. Oscar Wilde's "Intentions." Before we can discuss this theory, however, we are again forced to pass through a tedious process of acclimatization; for in Mr. Wilde's book we find ourselves at first in a strange country, a "Wonderland" like Alice's, where the truth appears through its logical opposite. It is, perhaps, not too much to say that Mr. Wilde's only medium of communication is the paradox — and a kind of paradox which strongly recalls his Irish origin. Where another advances by logical steps, slowly leading the reader to the comprehension of something he has not known before, at no point perplexing or confusing him, Mr. Wilde chooses another course. His first object is to bewilder. What he states is the exact opposite of the accepted truth. One's eyes grow large with wonder, or one smiles with an easy contempt and declines to give further time to the discussion of a thesis so ridiculous. On the very first page, for instance, of the dialogue, half Platonic, half after Renan, on "The Decay of Lying," we find this: "Enjoy nature! I am glad to say that I have entirely lost that faculty. . . . My own experience is that the more we study Art, the less we care for Nature. What Art reveals to us is Nature's lack of design, her curious crudities, her extraordinary monotony, her absolutely unfinished condition." A statement at first thought so astonishing and almost revolting as this is shortly followed by others scarcely less surprising. Many a man, says Mr. Wilde, who might have made

a good artist, is spoiled by falling into careless habits of accuracy. It is far easier to do a thing than to talk about it. Action, he declares, is something blind, "dependent on external influences, and moved by an impulse of whose nature it is unconscious. It is incomplete in its essence, because limited by accident, and ignorant of its direction, being always at variance with its aim. Its basis is the lack of imagination. It is the last resource of those who do not know how to dream." Statements like these, seemingly so flippant, prejudice the casual reader against Mr. Wilde, especially when his style is so florid that he might almost be accused of following up Irish bulls with Irish blarney. But such faults of taste and of style, disagreeable as they are, I do not hesitate to call unessential. Mr. Wilde is best when, quitting the extravagant paradox and its luscious development, he writes plain prose. But, after all, that is neither here nor there. We do not care so much how an author writes at his worst, provided that, like Mr. Wilde, he has after all something important or interesting to tell us. In this case it is the author's theory of art, or more particularly his theory of literature, that arrests our attention and holds it throughout. Here, as we have said, Mr. Moore differs widely from his two contemporaries. He follows his own strong bent, without seeing the necessity for a system of ethics or of æsthetics. But Mr. Wilde and Mr. Howells have both built theories for themselves. Each system is the exact opposite of the other; if brought before the general reader's tribunal of common sense, each will, I think, be found right, and each wrong. Both are thoroughly interesting.

Mr. Wilde's main thesis is that the decay of the art of lying accounts for the commonplace character of our contemporary literature. Where older authors have given us fiction for facts, those of today give us fact for fiction. They pore studiously over books, and shamelessly work up their subjects. As a result, we have historical novels like German treatises, realistic novels like the reports of a statistical society, and poems bolstered up with foot-notes. Thus many, who have in them the making of splendid liars, fall finally back on personal reminiscences, and feel the need of corroborating their facts. Nor are foreign literatures better off. M. de Maupassant tears away the veil from life, and makes it ridiculous and ineffective. M. Zola aims at nothing else but the reconstruction of life from "human documents" — of low life, with vices and virtues equally

commonplace and uninteresting. M. Bourget displays a fine subtlety in analyses of human nature, forgetting that at bottom we are all alike, and need not to be reminded of it. There are, of course, a few exceptions: Meredith, for instance, who has by deliberate choice made himself a romanticist, following the model of Balzac, who created life and disdained to copy it. But, as a rule, the modern authors are all in the wrong. They choose the wrong subject-matter; they give it the wrong treatment. Art is something very different from what they suppose. It lies outside of our lives, and scarcely touches them — a land of pure beauty, where nature has little or no part. For nature and art are antipathetic; it was nature that gave Wordsworth "Martha Ray" and "Peter Bell;" it was art that gave him the ode and the fine sonnets.

To be sure, life and art have certain relations. "Art begins with abstract decoration, with purely imaginative and pleasurable work, dealing with what is unreal and non-existent. This is the first stage. Then life becomes fascinated with this new wonder, and asks to be admitted into the charmed circle. Art takes life as part of her rough material, re-creates it, and re-fashions it in fresh forms; is absolutely indifferent to fact; invents, imagines, dreams; and keeps between herself and reality the impenetrable barrier of beautiful style, of decorative or ideal treatment. The third stage is when life gets the upper hand, and drives art into the wilderness. This is the true decadence, and it is from this that we are now suffering." What better historical instance can we have than the English drama? The miracle plays were abstract and theological; life scarcely entered into them. "But with the Elizabethan era the drama took on a language full of resonant music and sweet rhythm, made stately by solemn cadence or made delicate by fanciful rhyme, jeweled with wonderful words and enriched with lofty diction. She clothed her children in strange raiment and gave them masks, and at her bidding the antique world rose from its marble tomb. A new Cæsar stalked through the streets of risen Rome, and with purple sail and flute-led oars another Cleopatra passed up the river to Antioch. Old myth and legend and dream took shape and substance. History was entirely rewritten, and there was hardly one of the dramatists who did not recognize that the object of art is not simple truth, but complex beauty. In this they were perfectly right. Art itself is really a form of exaggeration; and selection, which is the very spirit of art, is nothing more than an intensified mode of over-emphasis."

Art, then, according to Mr. Wilde, is the artist's lie, his deliberate feigning of what is unreal. Life gives the artist the clue which he follows, the stuff on which he in modeling bestows the beauty that charms for an hour, a lifetime, or forever. Life, the truth, the reality; art, the lie, the unreality — this is Mr. Wilde's thesis, so brilliantly defended by him. And why not? Is not this "feigning" of the artist indubitably at the very foundations of all branches of literature, — historically, I mean? And are not we, impassioned for that shadow of reality which incites all the better workmen of today, at home or abroad — are not we drifting away from one great side of art, that which is least concerned with life? "The literary spirit, in its full grace and vivacity," says Sainte-Beuve, "consists in being able to interest one's self in that which is delicately pleasing in reading, in that which is otherwise useless for one, in that which is of no practical service in the eyes of the vulgar, in that which pleads no positive cause, in that which is only the ornament, the flower, the immortal superfluity, of society and of life." "Literature," he says elsewhere, "implies leisure, curiosity, disinterestedness, a breadth of taste, even caprice." In this conception of literature Mr. Wilde and Sainte-Beuve agree. And why should we not agree with them? Why should we not take literature as it is? It is and always has been a kind of ballet, as it were, a continual whirling and posing, with dainty music and softly colored lights, and all that is graceful and charming. But if literature is this, is it always this, this and nothing else? To such a question Mr. Howells answers in his "Criticism and Fiction."

The section of Mr. Howells's book which deals with criticism is not the most interesting part of it. On that subject he has already made declaration of his faith that critics are often prejudiced and uninformed persons, who write in unsigned articles more bitterly than they would dare to do over their names. But he has said this with more acerbity of manner than is customary with him; and, by way of rejoinder, some conspicuous critics of the press, forgetting the dignity of their calling, have bombarded him with somewhat offensive epithets. The controversy does not concern us here. Those in America who are most thoroughly interested in literature recognize Mr. Howells as a man of letters with unusually broad scholarship and wide experience, whose opinions, especially on matters of contemporary literature, though often stated with some extravagance, are uniformly worth consideration. It is the "fiction" part of the little vol-

ume which is valuable. In it the author thrusts aside, once for all, the theory which in Mr. Wilde we have just found admirable — that of art for art's sake. "To spin a yarn for the yarn's sake — that is an ideal worthy of a nineteenth century Englishman, doting in forgetfulness of the English masters, and groveling in ignorance of the continental masters; but wholly impossible to an American of Mr. Henry James's modernity. To him it must seem like the lies swapped between men after the ladies have left the table, and they are sinking deeper and deeper into their cups and growing dimmer and dimmer behind their cigars." We have, then, a fundamental opposition. Mr. Wilde laments that the art of the imagination, the art of lying, is decaying. Mr. Howells rejoices that from lies men turn to truth; that from the works of the pure imagination, or of those touched with the mania of emotional romanticism — from Scott with his mediæval ideals, from Thackeray the caricaturist — the main current of our contemporary literature turns in the direction first pointed out by Miss Austen. She was "the first and the last of the English novelists to treat material with entire truthfulness." After her, by fault of the mania for romanticism, the art of fiction visibly declined in England, as on the Continent, until recently there have sprung up in several countries men who have made friends again with nature and the truth, who write of what they see about them, taking the utmost care that their own fancy shall not color or heighten or in any sense idealize — any further than lies necessarily embodied in human nature — the common stuff of life. The art which is the willing lie these men disdain; their duty is to render account of the life about them "with simplicity, naturalness, and honesty." Of such an art not only the critics, but all men in whom humanity dwells, are competent judges. They "need not cast about for the instruction of some who profess to know better, and who browbeat wholesome common sense into the self-distrust that ends in sophistication," even though "they have been taught to compare what they see and what they read, not with the things they have observed and known, but with the things that some other artist or writer has done." The whole basis of criticism is changing. "The time is coming, I hope, when each new author, each new artist, will be considered, not in his proportion to any other author or artist, but in his relation to the human nature, known to us all, which it is his privilege, his high duty, to interpret."

I have let Mr. Howells speak for himself, because the earnestness and good sense of his words cannot fail to carry with them conviction, though they bear in themselves the seed for an opposition equally earnest and equally just. We have, then, three typical critics, the first a free lance in letters, with booty and pleasure throughout the district of his foray; the second and third, dogmatic knights on either side of the silver-golden shield. To change the figure into one which has a real similarity, the æsthetics of literature are not unlike those of eating and drinking. He who is wise will, with Mr. Moore, show a hearty zest for what comes to his table; like Mr. Howells, he will find most pleasure in fare which is simple and natural; and like Mr. Wilde, he will not disdain, at the proper time, a dainty dish.

AN APPARATUS FOR DETERMINING THE LIABILITY OF OILS TO SPONTANEOUS COMBUSTION.

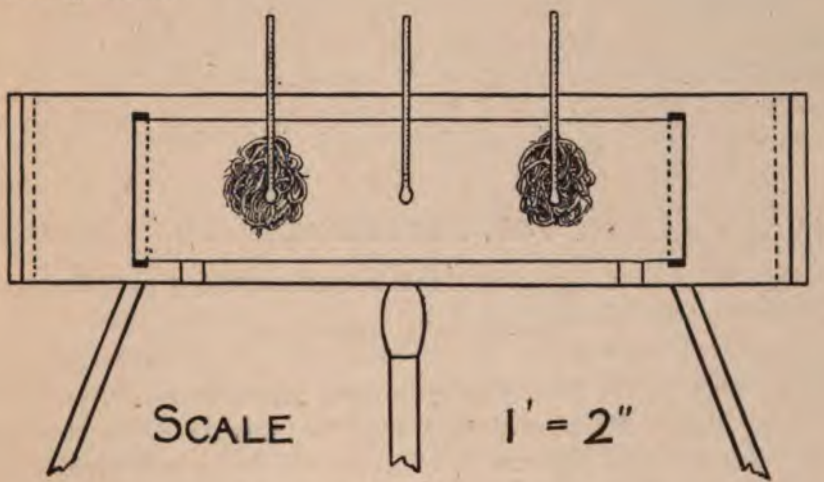
By ELLEN H. RICHARDS.

In 1877–79 the New England Cotton Manufacturers' Association and the Boston Manufacturers' Mutual Fire Insurance Company took up the question of the conditions under which the oxidation of oils, dye-stuffs, etc., in contact with easily inflammable materials, such as cotton in bales, cotton waste, rags, etc., was most likely to occur, with a view to better protecting themselves from loss and to insure a more efficient inspection. The problem was submitted to Professor Ordway, of the Institute of Technology, who suggested a form of laboratory apparatus to experimentally determine the facility with which oils oxidize under favorable conditions. From the results of the investigations of Mr. William B. Allbright, Class of '77, and Mr. Frederick W. Clark, Class of '79, a process was perfected which has been used by me ever since in determining all test cases which have come up in connection with this Insurance Company.[1] The first general conclusions were stated by Professor Ordway, under whose direction the experiments were carried on, in the Proceedings of the New England Cotton Manufacturers' Association, October 30, 1878 (No. 25). The final report was made December 1, 1884, in Special Report No. 18, Boston Manufacturers' Mutual Fire Insurance Company.

The requirements of an apparatus for laboratory use, *i.e.*, not too large, safe for use in an ordinary room, enabling a test to be made in

[1] At that time the only available records of experiments were those of Coleman in Glasgow and Dollfus in Alsace, published in the Bulletin of the Industrial Society of Mulhouse. The description of the apparatus and quantities used in these experiments was rather meager.

a working day of eight hours, easily managed and capable of giving uniform and reliable results, are fulfilled in the one shown in the following sketch:



The apparatus consists of a six-inch wrought-iron tube of thick gas pipe, two feet long, closed at each end by discs of wood; a four-inch tube of thin sheet iron closed at each end with an over-lapping metal cover is contained in this, leaving an inch air-space around it and three inches at the ends. Both tubes are perforated for the insertion of the three thermometers. The apparatus may be conveniently supported upon tripods, and is heated by a Bunsen burner placed midway between the ends. The entire apparatus may be inclosed in a shield if the place is draughty. The diameter of the inner tube permits the use of fifty grams of cotton waste (such as is used to clean machinery), to which is added an equal weight, fifty grams, of the oil to be tested. The oil is evenly distributed by careful manipulation, the waste rolled compactly but not too tightly, and pushed into the end of the tube, which it should fill so that gentle pressure is required to move it. The bulb of the thermometer is now carefully inserted into the middle of this ball. The metal cover, which should fit loosely so as to admit a little air, but affording protection from currents, is put on, and then the outer cover of wood. A blank of unoiled waste is placed under the same conditions in the other end. The thermometer in this blank should not be permitted to rise above 100° or 101° C. at the most. To have it reach this temperature, the middle thermometer must be

kept at about 125°. Since the balls of waste are equidistant from the source of heat, the necessary conditions are fulfilled if there are no draughts to blow the flame or cool one end of the cylinder.

In this apparatus anything which can be finely divided — shavings, leather, skeins of dyed yarn or rolls of dyed cloth — may be readily tested. Paper pulp, boards, and other carbonizible substances may be tested at a higher degree of heat for inflammability. Coal has not been subjected to experiment in it.

The results of greatest practical value obtained in the use of this apparatus have been, first, determining the cause of fires; and second, determining the degree of safety of the various oils used in manufacturing. Mineral oil, as is well known, is not liable to spontaneous combustion; and a certain percentage of animal or vegetable oil may be added to mineral oil without materially increasing the danger under ordinary circumstances. This percentage varies according to the oil; with neat's-foot and first quality lard oil some 50 to 60 per cent. may be used. With cotton-seed, not over 25 per cent. is allowable. The claim so often made for so-called "safe" oils, said to have been changed by special and secret processes of refining so as to be no longer dangerous, is easily exposed by this test.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY,
February, 1892.

BEAUMARCHAIS AND THE LOST MILLION.[1]

By ALPHONSE N. VAN DAELL,
Professor of Modern Languages.

While the Revolutionary struggle lasted, and even, to a certain extent, during Lafayette's life, the French influences which assisted the States in forming a nation were appreciated; but it has often seemed to me that the present tendency in many minds is to belittle the part taken by France, to ignore altogether the powerful current of the eighteenth century, which surely had some action in shaping the feelings of Americans like Paine, Jefferson, Franklin, and their contemporaries. It ought to be an interesting study to compare the Declaration of Independence and the Constitution of the United States with Rousseau's Contrat Social, for example, and mark the parallelisms which are visible even on a very cursory reading.

The building of legends diverging both from truth and justice has been going on, it seems to me, under English influences. Natural as it is that Americans should ever turn with love towards their mother country, to which so many hallowed remembrances must bring them, still it may be necessary to remind some that England is not the whole of the civilized world, and that they must guard against unconscious influences, the strongest of which is undoubtedly the community of language and literature. America has cut the political leading-strings that held her back; she has yet to efface more completely than in the past some of the intellectual influences and national prejudices that were necessarily a part of her early education. Among the prejudices which reign supreme in England is the contempt for foreigners; and I more than suspect that part at least of this prejudice subsists in minds like that of Mr. Stillé when he writes, in his article on "Beaumarchais and the Lost Million," these sneering words: "This letter [of Beaumarchais's], which is of great historical value as showing not

[1] An Address before a private club in Boston, February 19, 1892.

merely the nature of the *disinterested services* of France during the Revolution, but also the plan and motives of Beaumarchais . . ." (The italics are Mr. Stillé's.)

Now nothing in history is better proved than the disinterestedness of the motives which brought to the aid of the American colonies men like Lafayette; nothing more certain than the general aspirations for universal liberty and justice that prompted at that time the masses of the French nation. That the French government, which was surely not identical with the nation, may have perceived advantages of a different nature, and may have used America's hand for striking at the hereditary enemy, is also true beyond a doubt. But this consideration could hardly justify national ingratitude for services actually rendered; and that Beaumarchais did render invaluable service to the American cause at the most trying time is not to be denied.

Mr. Stillé says of him (I quote Mr. Stillé because he, being the most recent of Beaumarchais's accusers, has compounded in his pamphlet all that has been brought against him): " . . . He had been condemned by the Parlement de Paris to an infamous punishment for having produced in one of these lawsuits a receipt or discharge of a debt which that court had pronounced supposititious. . . . "

But the American writer fails to tell us by what intrigues, by what terrible abuse of power, this condemnation was obtained, and how public opinion reversed the decision of the judges, and soon after destroyed the corrupt tribunal that had sold injustice at the beck of the rich and powerful.

Mr. Stillé continues: " . . . To many he seems only a vain, ever active, unscrupulous intriguer, employing without hesitation lying and mystification whenever necessary to accomplish his object, which is assumed to have always been his self advancement and the gratification of an inordinate vanity for making himself talked of. By others he is thought chiefly responsible for the success of two revolutions, that of France . . . and that of America, by the energy which he exhibited in supplying us with arms and clothing for an army of twenty-five thousand men — supplies which we must admit were essential to our military success against Great Britain. . . . "

And a little farther we find the significant sentence: " . . . And for his aid to America in the hour of her sorest need, whatever may have been his motive, or however questionable may have been some of his proceedings, we should never cease to be profoundly grateful."

Let now Mr. Stillé bring forward his accusation. Let us drop the question of Beaumarchais's motives; this is an idle one, since it is difficult enough to establish facts, let alone the inner workings of our hearts. Unless the accuser makes out a very clear case against Beaumarchais, the man who *did* render the service he claimed at some risk of his life and the much greater risk of his fortune and who surely had *not* been the gainer by his transactions, we come inevitably to the conclusion that the odium of quibbling rests with the American Congress. But Beaumarchais's cause need not be left at this point; and in order not to do him the least favor, let Mr. Stillé explain the debated point:

"To this man our forefathers were told, in the early days of the Revolution, to look for succor and safety. The more they heard about him, the more completely did his position seem a mystery and riddle to them. To their sober and practical minds it was hard to conceive of him as a beneficent fairy, who, unsolicited, was willing to send us millions of dollars' worth of the supplies we most needed to carry on the war, and who, as they were told by Arthur Lee, the earliest American Commissioner in Europe, never suggested that he was to be paid for them, but merely hinted that it would be well for Congress to make him certain shipments of tobacco to conceal his transactions from the English. The Americans had no experience of merchants who carried on business in this way. It was not their interest, however, to inquire too closely into the source from which these supplies came, and they were inclined to settle down into the belief that Beaumarchais was a convenient *prête-nom* for the French government, which had so many reasons for encouraging us in our resistance, and so many more to conceal the aid it was giving us in our struggle with Great Britain. Whatever the Secret Committee or the Committee on Commerce of the old Congress may have thought or suspected, they did not, so long as the military supplies were provided in reasonable abundance, inquire with too much curiosity into their source. They accepted them with thankfulness, not doubting that they would discover in due time to whom they were indebted for them. In the mean time they chose to regard them as gifts from the King of France. For more than two years and a half they were under this pleasant delusion. They were confirmed in the opinion expressed by Arthur Lee, by letters written by the American Commissioners in Paris, who told the

Secret Committee in October, 1777, that they had been assured that these supplies were intended as *don gratuit*, or a voluntary gift on the part of the French government. Of course, under these circumstances, they made no effort to pay for them. Beaumarchais, under the name of Roderigue Hortalez & Co., tired of waiting for remittances from this country, sent an agent here in the beginning of the year 1778, who, in a tone very different from the high-flown rhetoric of his master's letters, demanded full payment for all the supplies which had been sent. This demand dispelled all dreams about *don gratuit* on the part of France. After ascertaining from the French government that Beaumarchais, under the name of Hortalez & Co., was really the man they had been dealing with, Congress, in January, 1779, made a partial settlement with his agent, and directed their President to write a letter of thanks and of apology to Beaumarchais for their delay in paying him. He received a large sum on account at that time, but a final settlement was delayed until further information from France reached them."

Let us stop here one moment: Why in the name of common sense, if to the sober and practical minds in Congress it was so hard to conceive of Beaumarchais as a beneficent fairy, if all dreams about the *don gratuit* were dispelled, if the President wrote a letter of thanks and apology to Beaumarchais, why not pay him at once, and why delay until further information should come from France?

But let Mr. Stillé continue: "While the liquidation was slowly going on, the French government, in 1781, entered into a treaty with our Commissioners in Paris, by which it agreed to loan to the United States a certain sum of money. In the preamble to this treaty there was a recapitulation of the sums paid by the French government for our use and account, but regarded as voluntary gifts, prior to the treaty of alliance in 1778, and an explicit statement was made that these sums, amounting to three million of livres, were to be considered as an absolute gift from the French King to us, and we were released from all liability therefor. It was discovered some time afterwards that our Commissioners had been paid by the French government not three millions, but two millions, prior to the treaty. Inquiry was of course made in France as to the person to whom this million had been paid. That government, for reasons which will be explained hereafter, refused to give the name of the person to whom payment

had been made, but it gave the date of the payment, June 10, 1776. The accounting officers of the treasury believed that this particular million was a portion of the secret service money paid by France to Beaumarchais for our use on the 10th of June, 1776, and that it had been expended in the purchase of supplies sent us, and as this million had subsequently been made by the action of the French government a present to us, they felt that Beaumarchais should not be paid twice for the same service, and thus they charged it against him in the settlement of his claim.

"Beaumarchais protested against such a decision. A controversy ensued, which lasted until 1835, in which the questions were, Whence came this 'lost million,' and what had become of it; by whom had it been used, and for what purpose? — questions between ourselves and Beaumarchais in the first instance, and afterwards between the French government and ours, which urged in his name his claim upon us. These questions involved interminable discussions about the rights and obligations which arose out of our secret diplomatic arrangements with France during the Revolution. They were the subject of many conflicting reports from committees of Congress for more than fifty years; two Committees on Claims, two Select Committees, and two Attorney-Generals recommended the payment of the claim, while two Committees on Claims and one Select Committee reported against its payment.

"It seems to me important for the good name of the country, as well as for the truth of history, that these transactions and their motives should be carefully examined and the fullest possible light thrown on them. As a matter of curiosity it is certainly worth knowing what became of this lost million, the origin and destiny of which were so carefully covered up by the French government. But the interest in such a discussion is of a wider and more permanent kind, for it embraces not merely a view of the manner in which military supplies essential to the prosecution of the war of the Revolution were obtained, but how far they were voluntary gifts bestowed upon us by France to secure her own ends in our quarrel, and how far we showed ourselves duly grateful for them. Our government has been branded with something worse than ingratitude — with dishonesty — in its persistent refusal to pay Beaumarchais this 'lost million.' Such an impression was doubtless made on the French government by our course — judging from the voluminous correspondence which was kept up for so

many years between that government and our own — and our conduct was long spoken of as an illustration of the old saying that republics were not only ungrateful, but shifty and tricky in their dealings."

I regret that I cannot review here the whole history of the transactions with Beaumarchais, for with him as an actor the direct documents teem with life and interest. But this would take an amount of time which is not at my disposal.

Let us then confine ourselves strictly to the so-called lost million; that is, to the question whether the million livres-tournois paid Beaumarchais, June 10, 1776 (for he is the person to whom the sum was given), was intended to pay for the ammunition he had sent or was to send to America. Beaumarchais and all the successive agents of the French government have denied it, while the defense for the refusal to pay the sum must needs rest on the affirmative answer.

Beaumarchais's receipt is worded as follows:

"J'ai reçu de M. Duvergier, conformément aux ordres de M. le Comte de Vergennes, en date du 5 courant, que je lui ai remis, la somme d'un million dont je *rendrai compte* à mon dit sieur Comte de Vergennes.

"CARON DE BEAUMARCHAIS.

"Bon pour un million de livres-tournois.
A Paris ce 10 juin, 1776."

The question is one very difficult to solve; that is, it may be forever impossible to find a written proof of the reason why the payment was made. The French government at the time had excellent grounds for not desiring to have its own doings known; and, generally speaking, even in our days of free inquiry, governments do often reserve funds for the disbursement of which they do not wish to give account to outsiders. Now it is a fact of public notoriety that Beaumarchais had been in relation with Count de Vergennes, and before him with King Louis XV, and that he had successfully concluded the several secret missions with which he had been entrusted. Whether he had taken to himself more credit than he was entitled to, is not to the point; and it may not be unnecessary to say that I do not consider Beaumarchais to have been a saint, nor are we to sit in judgment over the relaxed morals of his age.

Let us simply examine the circumstances, for we are reduced to circumstantial evidence; this evidence appears to me so strong, so decidedly favorable to Beaumarchais, that it is hard to understand how candid and unprejudiced minds could hesitate. I believe the case to be this: When the French government, yielding to the representations of Beau-

marchais and other men, resolved, for political reasons of their own, to assist the struggling colonies, then, as Stillé and others acknowledge, in the hour of their sorest need, Count de Vergennes understood very well that the help must be indirect, and that under no circumstances must it become known that France, while at peace with England, had taken an underhand part. He asked Beaumarchais to become his intermediary. Now the latter had an adventurous mind, to be sure; but did it ever occur to Mr. Stillé and his predecessors that the enterprise, judging it from a commercial standpoint, involved a great many risks — many more than a cool-headed business man would ever care to assume? For instance, the States might succumb in the struggle, or even if they triumphed might become insolvent for years to come, might even go into bankruptcy, or payment might be delayed, or they might refuse to accept Beaumarchais's cargoes, or the English might capture his vessels, or his ships might be drawn into naval battles and come out damaged. Not one of these suppositions would have seemed extravagant at the time of Beaumarchais's contract, and in fact the last three were realized. Whatever money Beaumarchais obtained, after the most vexatious delays, was paid not in cash, but in promises payable three years after, and some enterprising financial officers of the States inquired even about the means of not paying the notes when they became due.

Suppose, now, that Count de Vergennes, wishing to induce Beaumarchais to accept all the responsibilities resulting from his actions, had promised him *one million livres-tournois*, not as payment for any special purpose, but simply as an indemnity for any claims Beaumarchais might have against the French government for assuming the risks of business. This sum could truthfully be said to have been spent for the American colonies, and yet could not be considered as a partial payment for the ammunition that Beaumarchais sent over. This theory is, I believe, confirmed by all the facts known to me; it agrees with the repeated affirmations of the French government, of Beaumarchais, with the wording of the treaty between France and the United States, as I shall attempt to prove, and is in contradiction with the testimony of but one single witness, Arthur Lee.

This personage, whom Jared Sparks in his life of Benjamin Franklin (see page 447 and following) calls an uneasy and violent character, jealous and distrustful, causing quarrels and difficulties wherever he goes, is at the bottom of Beaumarchais's troubles. Some idea of his

high self-appreciation may be obtained from his first confidential letter to the Secret Committee of the Congress, dated January 3, 1777: "The politicians of this court are in a kind of trembling hesitation." You could never guess why! "It is," continues Lee, "because the promises made me by the French agent in London have not been entirely kept." Imagine the King of France and his hosts trembling in the presence of Arthur Lee!

In another letter Lee has the audacity to write that M. de Vergennes and his secretary have repeatedly assured him that no return was expected for the cargoes sent by Beaumarchais. And this was after Silas Deane had written to Congress (December 29, 1776) that Beaumarchais has trusted the United States for a considerable amount, and that he confidently expects them to repay him promptly. Lee, for reasons of his own, which I do not pretend to judge, affirmed a thing which he and Silas Deane and Benjamin Franklin knew to be false. This is sufficient to show the animus of the man. A letter of Beaumarchais of December 20, 1777, will serve to explain what he felt, and what was going on:

"Je ne sais si je me flatte, mais je compte sur l'honnêteté, sur l'équité du Congrès comme sur la mienne et la vôtre. Ses députés ici ne sont pas à leur aise, et le besoin rend souvent les hommes peu délicats: voilà comment j'explique l'injustice qu'ils ont essayé de me faire. Je ne désespère pas même de les ramener à moi par la douceur de mes représentations et la fermeté de ma conduite. Il est bien malheureux, mon ami, pour cette cause, que ses intérêts aient été confiés à plusieurs personnes á la fois, un seul eût bien mieux réussi, et pour ce qui me regarde, je dois à M. Deane la justice qu'il est honteux et chagrin à la fois de la conduite de ses collègues avec moi, dont le tort appartient tout entier à M. Lee."

Later on, Lee is the only man whom Beaumarchais refuses to accept as an umpire, and it is Lee who, of all men, is chosen by the Congress to examine his claims.

That the French government must have, as I suggested, guaranteed Beaumarchais against certain losses, is shown by a letter of his to the King, Louis XVI, dated September 11, 1779. Beaumarchais's ships had taken a glorious part in a battle against the English fleet, and there were pressing needs of money which he exposed. The King granted his request for a loan of four hundred thousand livres, which was repaid later on. Beaumarchais was also paid for the actual loss of his merchantmen, which had been scattered in consequence of the battle. This transaction shows, it seems to me, that an agreement must have existed between the French government and Beaumarchais, and the conclusion is very near that the lost million was included. On

January 15, 1779, John Jay, President of the Congress, writes from Philadelphia to present the thanks of his country to Beaumarchais, regrets the misfortune which he has undergone for the defense of the States, and promises prompt payment.

On September 3, 1778, W. Carmichael writes a most friendly letter, in which he assures him of his friendship, and takes sides with him in his struggle against Lee and for Benjamin Franklin. In October, 1779, the Congress sends Beaumarchais two million five hundred and forty-four thousand livres in three-year promises. This was perhaps taking too literally Count de Vergennes's words, who had told the American envoy, three years before, that he need not hurry with the payment of the claims.

There was some afterthought in the minds of Congressmen, for a little later we hear of Robert Morris inquiring of Benjamin Franklin whether there was not some means of avoiding the payment of these promises. And it is astonishing to see Franklin, after being urged by M. Darival for the reimbursement of the French advances, ask again whether Beaumarchais is to be paid. The French officials answer, of course, that they do not know anything about the Beaumarchais claim. In 1781 Silas Deane — who had been on the spot since the first and watched every transaction — having returned to France, finds in his summing up that the States owe Beaumarchais three million six hundred thousand livres. In 1783 a new agent is sent by the United States, Mr. Barclay; he refuses to accept Silas Deane's accounts, and in spite of Beaumarchais's protest, proceeds to revise once more. He reduced the amount admitted by Silas Deane. But the States did not pay anything, and now an incident came up which was to be the cause of the indefinite postponement of the Beaumarchais claim.

The United States were already owing considerable money to France, and still asking for a further loan of six million livres. It was agreed that the whole financial situation was to be cleared up by an exact statement in the contract of all sums previously received, either as a loan or as a gift.

As loans were enumerated eighteen millions; then ten millions obtained in Holland but guaranteed by France, and on which the King of France was paying interest; and finally the new loan of six millions, making together thirty-four millions which were to be refunded.

As gifts, that is to say as sums which the King had used for the Americans but for which he did not claim anything, were enumerated

first, three millions given before 1778, and second, six millions given in 1781. Now the American agents had received only two millions instead of three, as Franklin singularly enough did not remember until 1786. But this very fact proves that neither Americans nor Frenchmen at that time considered that any consequences should be drawn from that declaration, which was meant only as a kind of memorandum of the sums expended, for which the King did not wish to receive reimbursement. Franklin had received two millions directly from the French treasury, and one million through the intermediary of the Fermiers Généraux, on account of which some one hundred and fifty thousand livres had been paid in tobacco. But after inquiry the French government answered that it had nothing to do with that million, but that one of the millions referred to in the second item had been paid to a person not to be named, on June 10, 1776.

Allow me a short parenthesis which will make clear the way in which money matters seem to have been then treated by the United States government, and which goes far to explain the Beaumarchais controversy. Franklin claimed interest on the one hundred and fifty and odd thousand livres paid by the American government, but no mention was made of the interest which was evidently due to the Fermiers Généraux on the eight hundred and fifty thousand livres making up the round million, so that the Americans expected interest on the partial payment of a sum they owed, while not paying themselves any interest on the principal. In a letter of the French Chargé d'Affaires, M. de Marbois, to Count de Vergennes, dated Philadelphia, August 24, 1784, we find the following characteristic appreciation:

"I do not believe M. M. [evidently Mr. Morris, Secretary of the Treasury] capable either of love or aversion for any power, but I have good reason to think that his greed may lead him into most reprehensible irregularities, and that, unless bound by instructions from the Congress, he will care very little to fulfil the obligations of the United States toward His Majesty the King of France."

The French minister refused, on September 16, 1786, to communicate what we know to be Beaumarchais's receipt for the million, or even to name the signer, and the Congress now concluded, first, that Beaumarchais had received the million; second, that this million ought to be refunded by him; third, that the Congress was not bound to pay anything until the mystery had been explained. While this was going on, Beaumarchais, growing desperate, asked at least for

submission of his claims to umpires, being willing even to let the Americans name them all, provided only that Arthur Lee should be excluded.

On the 12th of June, 1787, he wrote to the President of Congress as follows:

"Que voulez-vous, Monsieur, qu'on pense ici du cercle vicieux dans lequel il paraît qu'on s'enveloppe avec moi? Nous ne ferons aucun remboursement à M. de Beaumarchais avant que ses comptes ne soient réglés par nous, et nous ne réglerons point ses comptes pour n'avoir point de remboursement à lui faire! Un peuple devenu puissant et souverain peut bien regarder, dira-t-on, la gratitude comme une vertu de particulier au-dessous de sa politique; mais rien ne dispense un État d'être juste et surtout de payer ses dettes. J'ose espérer, Monsieur, que, touché de l'importance de l'affaire et de la force de mes raisons, vous voudrez bien m'honorer d'une réponse officielle sur le parti auquel l'honorable congrès s'arrêtera, soit de me régler promptement et de solder son règlement, comme un souverain équitable, soit de choisir enfin des arbitres en Europe pour juger les points en débat *d'assurances et de commission*, ainsi que M. Barclay eut l'honneur de vous le proposer lui-même en 1785, soit enfin de m'écrire sans détour que les souverains d'Amérique, oubliant mes services passés, me refusent toute justice: alors j'adopterai le parti le plus convenable à mes intérêts méprisés, à mon honneur blessé, sans sortir du profond respect avec lequel je suis, du congrès général et de vous, Monsieur le président, le très-humble, etc.

"CARON DE BEAUMARCHAIS."

Congress answered by naming Arthur Lee to examine the claim. He found not only that Congress owed Beaumarchais nothing, but that the latter was indebted to the United States for the sum of one million eight hundred thousand livres. It is true that Mr. Alexander Hamilton, revising the account, found, in 1793, that Beaumarchais was a creditor for two million two hundred and eighty thousand livres; but he concluded that it was not advisable to pay the claim until the French government should have been asked again for that now celebrated receipt.

In consequence, Gouverneur Morris requested Buchot, then Minister of Foreign Affairs, on June 21, 1794, for the desired communication; and the latter, actuated probably by the desire of doing the reverse of what had been done during the Ancien Régime, surrendered it. The Congress immediately asked Beaumarchais to prove that the million which he had received was not intended for the United States. The reply followed immediately:

"Je demande qu'il me soit donné acte de la déclaration la plus précise que je fais, que jamais je n'ai reçu du roi Louis XVI, de ses ministres, ni de personne au monde, *ni un million, ni même un seul shilling pour vous être offert en présent;* que tout l'or que j'ai employé pour vous servir, en ami bien zélé, en loyal négociant, et au seul titre d'un commerce équitable, n'a été rassemblé par moi, tant en France qu'en d'autres États de l'Europe, qu'à titre d'association *d'emprunt ou de circulation;* que tous mes créanciers, moins patients envers moi que je n'ai dû l'être envers vous, ne m'ont pas laissé vingt années sans exiger leur compte et leur acquittement, et que s'il *m'en restait quelques-uns à solder, question qui vous est étran-*

gère en qualité de débiteurs, ce ne serait qu'une obligation de plus pour vous de me mettre en état de le faire en vous acquittant envers moi. Quant au contrat de 1783, dont vous m'apprenez l'existence et que j'ai toujours ignoré, je déclare que ce contrat, où je n'ai pas été appelé ni par vous, ni par les ministres de France, m'est absolument étranger, sous quelque point de vue qu'on l'envisage, par cela seul que je n'y ai point été appelé, ce qui était indispensable, si vous deviez, après douze ans, essayer de vous en faire un titre pour éluder ou éloigner mon payement, après avoir épuisé tous les autres."

The last letter which the dying Frenchman wrote to Congress again demanded justice — no longer for him, but for his daughter, and for his widow. These latter claimed over four million, but asked in vain, at least for the sum which Alexander Hamilton had named. Talleyrand writes on the 28th of Germinal XI, and again in 1816 the Duke of Richelieu affirms to Mr. Gallatin, that the one million given to Beaumarchais on June 10, 1776, had nothing to do with the ammunition sent to America.

It is only in 1836, when the twenty-five million controversy and President Jackson's violence showed the French that the United States were not as patient creditors as they themselves had been, that they succeeded in having the Beaumarchais claim (reduced, it is true, to eight hundred thousand livres, a figure much below that which anybody had ever named) included in an international settlement.

It may be asked why I have taken up this subject. Let me tell you at once that it is not for the vain pleasure of complaining of the American nation, nor for the purpose of showing the United States in an unfavorable light. I do not believe that the case of Beaumarchais is unparalleled in the annals of history. I simply wished to call attention to the injustice done, first, because I think there is among the causes that led to it a narrowness of spirit, a kind of race prejudice which is far from dead, if we may judge from Mr. Stillé's book; second, because it teaches a lesson which may be valuable for the future. It is not best to entrust to a large body of men the solution of questions of personal justice, especially when the claimants have no political influence. The average politician is always anxious to have money spent in his district, where it will show his admiring constituents his power and influence. But when the money is to go far away, he is likely to think that he ought to be very economical; he will stop at no quibble; he will depend on the judgment of persons as careless and ignorant as he is himself, and refuse to see the injustice or harm he has done. *He* has done none in any case; it is the majority, and if *he* is made later to realize the mischief, it is easy for him to accuse others.

NOTES UPON THE ESTIMATION OF CHLORINE IN ELECTROLYZED SOLUTIONS.

By L. M. NORTON, Ph.D.

The numerous experiments made during the last few years upon the production of chlorine, hypochlorites and chlorates by the electrolysis of solutions of chlorides, have brought into the hands of analysts solutions containing chlorine in several combinations.

It has been found that rapid volumetric methods for the determination of the combinations in which the chlorine existed were a necessity where such solutions were to be frequently tested, and the writer thinks that a description of the methods he has employed, while they contain little that is new, may be of interest to chemists who deal with similar problems.

By the direct electrolysis of aqueous solutions containing sodium chloride at ordinary temperatures, solutions are obtained which contain sodium chloride, sodium hypochlorite and sodium chlorate, and this general action is undoubtedly true in regard to other chlorides. A most careful examination has failed to show the presence of sodium chlorite in solutions thus prepared. If, however, the temperature is raised to 50°–55° C.[1] the solution contains mainly sodium chloride and sodium chlorate.

Such solutions may be very conveniently examined by the methods described below.

ESTIMATION OF CHLORINE COMBINED AS HYPOCHLORITE.

A quantity, 25 cc. for example, of the solution is measured out and titrated in the ordinary manner with a sodium arsenite solution of known strength. The solution after the titration must be preserved for the determination of the chloride present.

From the equation :

$$As_2O_3 + CaCl_2O_2 = As_2O_5 + CaCl_2$$

the quantity of chlorine combined as hypochlorite may be calculated.

[1] Rigaut, La Lumière électrique, 40, 102.

ESTIMATION OF CHLORINE COMBINED AS CHLORIDE.

The portion of the original solution which has been titrated with sodium arsenite is now conveniently made up to a known volume and an aliquot part is taken. To this part a solution of pure potassium chromate is added, and the solution then titrated with silver nitrate of known strength. The presence of arsenic compounds does not interfere with this titration in any way; in fact, sodium arsenite may be used as an indicator in the place of potassium chromate, and equally good results may be thus obtained.

The figures furnished by the titration with silver solution give the chlorine present as chloride and hypochlorite together in the original liquid. By subtracting the chlorine combined as hypochlorite from the chlorine shown by the titration with silver solution, the chlorine present per cubic centimeter as chloride is readily obtained.

ESTIMATION OF THE TOTAL CHLORINE.

It might seem at first thought that the total chlorine could be obtained by the titration of the original solution which had been electrolyzed; but this is not the case, as during the electrolysis of a chloride solution there is a decrease in its volume caused by the rise in temperature due to the current, so that the contents per cubic centimeter of the solution before and after electrolyzing are not comparable, unless, in the latter case, the solution is carefully brought to its original volume by the addition of water. A further error is unavoidably introduced by a loss of chlorine due to its escape directly into the air during the electrolysis, a loss which the writer has always observed in the various forms of apparatus which he has used.

As the solutions under discussion contain chlorine in two oxidized forms as well as in the form of chloride, it is of course evident that for the determination of the total chlorine the simplest method will be the reduction of the oxidized chlorine compounds to the form of chloride, and a subsequent titration with silver solution. Various reducing agents have been recommended from time to time for the reduction of chlorates to chlorides with a view to the subsequent titration of the chlorides with silver solution. After many experiments the writer has found that the most satisfactory agent for this reduction is a saturated aqueous solution of sulphurous acid. This was suggested by Rose,[1]

[1] Handbuch der analytischen Chemie, 2, 613, Leipzig, 1871.

and lately Engel[1] has called attention to its possible use in this connection. Chlorates are completely reduced to chlorides by boiling for a few moments with an excess of an aqueous solution of sulphurous acid, with or without the addition of nitric or sulphuric acid. Thus the following figures were obtained in the analysis of a sample of commercial chlorate of potash by reduction with sulphurous acid solution, after acidifying strongly with nitric acid, boiling off the excess of sulphurous acid, followed by titration with silver solution in the manner to be mentioned below:

Theory for pure $KClO_3$.		Found.
Chlorine	28.93%.	28.70%, 28.82%, 28.82%, 28.79%

As during the reduction free acid is formed in the chlorate solution, even if it is not added at the beginning, the use of potassium chromate as an indicator, together with direct titration with silver nitrate solution, failed to give satisfactory results. The figures obtained, however, were entirely satisfactory if the reduced solution was treated with an excess of silver nitrate solution of known strength, ferric alum added as an indicator, and the excess of silver nitrate determined by titration with ammonium sulphocyanate solution in the manner recommended by Volhard.[2]

The total chlorine being thus obtained, the amount present as chlorate per cubic centimeter is easily found by subtracting from the total quantity the amount previously ascertained to be present in the forms of chloride and hypochlorite.

It is also practicable to determine the hypochlorite by means of an arsenite solution, the chlorine present as chloride and hypochlorite together by means of silver solution as given above, and the hypochlorite and chlorate by allowing them to act upon potassium iodide in the way ordinarily described in the handbooks; and from the data thus obtained the amount of chlorine present in each form can then be calculated. I consider this process less convenient than the one already described.

The writer has used still another method which gives good results, but it is less rapid than those mentioned. After the determination of the hypochlorite and chloride present has been made, the total oxidizing power of the original solution, due to the hypochlorite and chlorate

[1] Silva, Traité d'analyse chimique, 151, Paris, 1891.

[2] Annalen der Chemie, 190, 1.

contained in it, may be estimated by adding a known quantity to an excess of an acid ferrous solution, containing a known quantity of ferrous sulphate, boiling for a few moments in an atmosphere of carbonic acid, and titrating the unoxidized ferrous salt with potassium permanganate or potassium bichromate. The oxidizing power of the original solution can be calculated from the ferrous iron oxidized by it; and as the hypochlorite present is known, the chlorate present can be calculated.

ABSORPTION OF CHLORINE.

In testing the efficiency of electrolytic chlorine generators it is most convenient to absorb the chlorine produced in sodium hydrate solution. The amount of chlorine already present in the alkaline solution should be determined by a preliminary analysis. The alkaline solution which absorbs the chlorine will contain it mainly as hypochlorite, but also as chloride and chlorate. A known fraction of the absorbing liquid is taken, made up to 1,000 cc., and of this 50 cc. are measured and 20 cc. of sulphurous acid solution and 5 cc. of nitric acid added, the sulphurous acid removed by boiling, and the total chlorine is then easily found by the method described for the estimation of the total chlorine in the electrolyzed solution, and from this the chlorine originally in the solution must be subtracted.

I see no reason why the methods given above may not be applicable to the analysis of bleaching powder in cases where the estimation of total chlorine and chlorate is desirable.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY,
February, 1892.

MILK SUPPLY AND THE PUBLIC HEALTH.[1]

By WILLIAM T. SEDGWICK, Professor of Biology.

One of the noteworthy social changes of the present century has been the growth of cities and the relative increase of urban and suburban populations. These vary in density; but in all, or nearly all, cases they depart so widely from the more primitive rural or village form as to constitute an organization essentially altered and a condition in which the more primitive ways of life require to be much amended. This fact is quickly recognized in the social, legal, and moral relations; but on the physical, and especially on the sanitary, side it is often overlooked. The most fundamental sanitary effect of urban and suburban life is to make general and public the supplying of certain common wants which were previously special and private. Much that was essentially domestic, and under private supervision, becomes essentially foreign, and passes of necessity beyond domestic control. The water supply, once drawn from the domestic well or cistern, which was easily guarded, is now derived from a pipe filled, it may be, many miles away, from a source of which the individual householder has no knowledge whatever. The household wastes, once disposed of in the simplest fashion, now pass into common sewers through convenient openings in sinks and closets and sometimes sleeping-rooms. Even the supply of food becomes also essentially public. Animals are no longer slaughtered in the domestic barn, but in the public stockyard or abattoir. Flour, vegetables, fruits, and all the other necessaries or luxuries of life have passed, or are passing, beyond the pale of domestic supervision; and supervision by public experts must necessarily replace that of the individual or the family.

There can be no question that these changes have often brought with them marked sanitary improvements; but it is equally clear that

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[1] An Address read before the Society of Arts, Massachusetts Institute of Technology, December 10, 1891.

the possibilities of extensive harm are in some cases distinctly enhanced. The introduction of a pure water supply can easily be shown to be followed by an improvement in the public health; but the contamination of such a supply may be followed by disaster far more universal than could possibly have happened upon the more primitive system of disconnected domestic wells. Other examples might be given, but this will suffice to show that public supplies are attended with public dangers.

The corollary is plain. The public, in surrendering the privilege of private control and supervision of their various supplies, should carefully provide and maintain for them the most expert public supervision. This is supposed to be done in the case of water supply and sewage disposal, though it is seldom done as it ought to be. The sources of the public water supply are generally well known and more or less guarded against pollution. The removal of sewage and garbage is usually fairly well attended to. Meat and other provisions receive some public supervision, though more might well be given. Decayed fruit is occasionally seized and destroyed. Even milk is examined, and dealers in watered milk are prosecuted. In most of these cases more ought to be done than is now done to protect the public, that is, to protect the private individual who, having surrendered the right of domestic supervision, is virtually at the mercy of dealers in public supplies. In particular his water supply, sewage disposal, gas supply, food supply, and especially his milk supply, demand the most careful public supervision. In this paper we shall consider the problem of milk supply and the public health.

At the top of the animal kingdom stand those animals which give milk. This property is so peculiar that it has been made the distinctive characteristic of the highest group of animals, which are named, accordingly, the *mammalia*, that is, those animals which have *mammæ*— the mammary glands or milk-producing organs. In the process of the domestication of animals, primitive man appears to have cultivated and developed this remarkable property, using the cow, the goat, the mare, the camel, the reindeer, and other animals, not only as beasts of burden for mechanical purposes, but also as direct bread-winners in the form of milk producers. In one stage of his development, primitive man lived with his animals, and moved about with them in their search for food. In this simple, migratory state, he occasionally found lands flowing with milk and honey, and, untroubled by the purity or freshness of his milk

supply, sang their praises for future generations. To him the only problem of milk supply was that of quantity; its quality was, as yet, unaffected by the barnyard, by the stable, by adulteration, by staleness, or by the centrifugal machine.

With his departure from the primitive and patriarchal state, man has come, at length, to dwell in dense masses called cities — places which do not flow with milk or honey, but, instead, with smoke and sewage. Under these changed conditions man has changed, and his food supplies have changed; but not so much in the nature of the raw materials, as in the treatment to which they are subjected before they are consumed.

Most of our foods today, fortunately, are purified by fire before they are admitted into our alimentary canals. Meat is purified by roasting; potatoes, by washing, peeling, and heating to a high temperature; cereals are powdered, sifted, moistened, and then purified by fire, in the oven or in boiling water; bananas, apples, oranges, pears, grapes, and nuts come to us hermetically sealed in dirt-tight skins or shells; cherries, berries, and such vegetables as celery and lettuce, on the other hand, we eat as we find them, raw — but only after a thorough cleansing.

Milk, however, to a great extent, man still uses much as his primitive ancestors used it; namely, in the raw or uncooked condition. And yet the milk of a modern city is altogether different from the milk consumed by primitive man. It is seldom fresh; it is often adulterated, and it is usually dirty. The stable, and travel more or less prolonged, have had their effect, so that the milk when delivered is often far along on its way towards decomposition. Here primitive man had the advantage, not only in his roaming, which made the stable unknown and the barnyard an impossibility; the cow, the goat, and the camel were his valued companions, and their milk was consumed while it was still fresh. For him it was not transported hundreds of miles by railway, or deposited at the door of his tent at an hour of the morning suggestive of freshness, but really long after it had parted from the cow.

It is instructive to consider somewhat carefully the source of the milk supplied to a modern city, and its history, from the time it originates in the udder of the cow until it is consumed by the city infant or adult. Milk is an animal secretion prepared in much the same way as are the tears, the bile, the gastric juice, the urine, the sweat, and the

saliva. It is secreted in the mammary gland, or udder, by certain epithelial cells which manufacture it from the blood. During the process of secretion these cells break open and contribute of their own substance to the mixture of water, salts, sugar, and fat, which constitute milk. At every milking a portion of the udder substance, the actual animal stuff, is given off; and thus it comes to pass that milk is strictly an animal food, like an infusion of muscle or of liver. It is unnecessary to say more to make it plain that it is highly unstable, highly decomposable, highly putrescible.

As every one knows, milk is drawn from the cow by a peculiar operation called "milking." As it is ordinarily conducted, this act requires the operator to be seated before the udder for some moments, probably never fewer than five. A pail is held between the knees of the milker, the teats are seized by the hands, and by a peculiar peristaltic action the milk is squeezed out from the teat, which is instantly filled again from the secreting gland. The operation requires much shaking of the udder, and very vigorous movements of the milker's arms. Only those who know can realize the unclean condition of many milkers, many stables, and most cows. I need only remark that a wide-mouthed pail held under the shaken udder necessarily receives whatever is dislodged above it. The sediment in milk pails and milk cans has been examined microscopically and found to consist of hairs, dandruff, flakes of dried excrement, with less objectionable matters, such as hay, in large abundance.

The milk thus drawn, and already more or less charged with what may be euphemistically called "dirt," is next strained through a coarse filter of some sort, by which the larger particles are removed. It is then cooled to check the process of decomposition and thus increase its keeping qualities; and eventually it is shipped to the city, either by wagon or by railway. More or less of time here necessarily intervenes before the milk is finally placed in the proper cans and delivered early in the day to the consumer. In Boston the milk is usually from thirty-six to forty-eight hours old before it is thus delivered. In the consumer's hands several hours more often go by before the milk is actually used. In the case of people too poor to have ice, resort is usually had to groceries which keep one or more large cans of milk on ice. In some parts of Boston milk is doled out from such groceries by the cent's worth.

The amounts of milk supplied to cities are very large. In Lowell,

for example, about 30,000 quarts are received and sold daily. Lowell has 80,000 inhabitants, so that for every man, woman, and child, Lowell receives three eighths of a quart daily. Much of this is used, of course, for cooking; but much is also fed in the raw, uncooked condition, to infants, children, and invalids. The amount received in Boston is as much as 50,000 gallons per day. We may reasonably ask what is the origin and sanitary condition of this immense and important public supply.

A considerable portion of the Boston milk supply is produced upon farms adjacent to the city; a very small portion within the city itself; but the greater part originates upon distant farms in Massachusetts and New Hampshire, from which it is brought into the city by railroad. On its arrival it is sold to small dealers or peddlers, by whom it is kept for a longer or shorter time, and ultimately delivered to the consumers. The consumers, of course, have no knowledge whatever of its origin or previous history when it is deposited at their doors. They do not even know, in most cases, whether it has come from their own State or not. Obviously the consumer can exercise at present no control over the cows or the stable which produce his milk supply. He may, however, investigate its condition on its arrival.

It is well known that milk, especially as it is found in large cities, is usually swarming with living bacteria[1]. Most of these are probably harmless vegetables; nevertheless they constitute a host of organisms entirely foreign to the milk itself. The so-called "pure" milk ordinarily obtained in Boston, even when it has not been tampered with, frequently contains in a teaspoonful millions of these microscopic vegetables luxuriantly thriving in a soil most favorable for their development. Moreover, although themselves invisible, they do not fail to produce obvious effects. The souring and chemical decomposition of the milk which soon appear are due exclusively to their vital activity, and the milk which was originally sweet, and which in their absence remains forever sweet, becomes, owing to their activity, sour and chemically decomposed. In this condition it has important uses, but it is no longer "fresh" or "sweet" or "normal" milk. Under the worst conditions milk may actually be delivered to the consumer nearly or quite sour; and we have ascertained that in Boston it is

[1] The statements that follow are drawn chiefly from "A Bacteriological Examination of the Boston Milk Supply," by W. T. Sedgwick and John L. Batchelder, Jr. Boston Medical and Surgical Journal, January, 1892.

regularly and systematically delivered to the great majority of consumers comparatively well advanced towards this condition.

It is with this aspect of the subject only that we shall deal in the present paper. We shall show, in the first place, that the normal milk of the cow contains no ordinary putrefactive bacteria; we shall next seek to discover the source of the swarms of bacteria which infest ordinary city milk; we shall then briefly consider the bearing of these phenomena upon the public health, and the possibility of remedying the present conditions.

It will be observed that we are not now dealing with the specific germs of disease. It is well known that milk is an excellent food for many kinds of bacteria, and this, added to the fact that it is one of the cheapest and most trusted of food-stuffs, makes it, when infected with pathogenic organisms, a peculiarly dangerous vehicle for the transmission of infectious diseases. Cow's milk, when once infected with the specific organisms of disease, is probably a medium far more dangerous than an equal volume of infected water, since, for many bacteria, it is a much more favorable soil. With this aspect of the subject, however, we shall for the present have little to do. We desire first simply to show how far the milk in Boston regularly differs in bacteria from the normal, and to discover the causes of this difference. The methods employed for this more general inquiry have not been adapted or intended for the discovery of the exceptional cases of contamination of the milk with the specific germs of such diseases as tuberculosis or typhoid fever.

Chemical examinations, both official and unofficial, of the milk supply of Boston, have been, and continue to be, repeatedly made; but no systematic and extensive bacteriological examination has hitherto been reported. It appeared to us therefore highly important, at least from the scientific standpoint, to inquire what information might be gained by a series of bacteriological examinations systematically conducted. To this end, during the past two years we have made many hundreds of examinations of Boston milk, of which only the principal results are here published.[1]

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[1] An abstract of the original paper was read by J. L. Batchelder, Jr., before the Boston Society of Medical Sciences, March 31, 1891. The data upon which it was based were largely obtained as results of an investigation conducted under my direction by Mr. Batchelder, and presented by him, in 1890, as a thesis for the degree of Bachelor of Science in the course in Biology at the Massachusetts Institute of Technology.

In order to discover the bacteriological changes which milk undergoes in its journey from the cow to the consumer we have made a series of examinations of milk as it flows from the cow; of milk as it is carried from the stable; of milk as it is found upon the tables of well-to-do country families; of milk as it arrives in Boston; of milk as it is sold from wagons, in groceries, etc.; and finally, of milk as it exists upon the tables of well-to-do city families. The main object throughout was to discover the source and significance of the bacterial swarms which infest city milk. To this end our experiments began with the cow.

I. NORMAL (UDDER) MILK.

That milk must be regarded as *normal* which is drawn from the teat of a healthy cow. In such normal, or udder milk, we have found no trace whatever of bacteria. The same result has been obtained by some previous observers, while others have found it difficult or impossible to obtain milk from the cow free from all germs. All observers, however, agree that very few, if any, organisms are present in normal cow's milk, and that the germs sometimes observed have probably been due to defective manipulation. Without special precautions, we also have found it extremely difficult to draw milk from the cow's udder *entirely* germ-free, and at first we failed to get it absolutely sterile. By a special method, however, we have succeeded in drawing, with ease, from the cow's udder, absolutely sterile milk. From this fact and the similar observations of others, there need be no question that the normal milk of the cow is free from bacteria. A consideration of the mechanism and physiology of secretion will show that this, for the healthy cow, is entirely reasonable. Normal cows secrete milk; they do not secrete bacteria.

A method by which this fact can be easily established deserves description. If one seeks to draw normal milk, germ-free, by hand, and milks in the ordinary way into a sterilized bottle, he will usually fail to obtain sterile milk. The reason appears to be that the act of milking shakes the udder and throws down from the skin of the animal a shower of dust and germs which pass into the bottle with the milk. By the use of the silver catheter or so-called "milking-tube" of veterinarians, however, this disturbance is completely avoided. The milk flows, without action of any kind on the part of the observer, in a strong and steady stream, of which a portion may be easily caught by a merely instantaneous exposure to the air.

The precise method of procedure is as follows: A clean cow is selected, and the operation should be attempted only in a clean stable. Care being taken to avoid disturbance of the bedding (if any), the skin of the hind quarter and that of the udder are washed. The teats are then wiped dry, and milking in the ordinary fashion is begun in order to start the secretion. A pause is then made, and while the dust of the stable settles, the milk accumulates in the udder. The catheter, previously sterilized by heat, is drawn from a plugged test tube and cautiously passed through one of the teats into the udder. The milk instantly flows away from the resting udder, through the catheter, in a strong and continuous stream, from which a series of samples can readily be taken. Many of these, in our experiments, proved, if planted at once, perfectly sterile. Others showed a very few colonies, which were plainly due to aërial contamination.

We have not sought to determine the presence or absence of tubercle bacilli in normal milk, as this was foreign to our investigation. At the same time it is clear that this method might well be applied in such an investigation. The detection of tubercle bacilli in milk has hitherto depended, not on cultures, but upon the differential staining of cover-glass preparations, and upon the infection of guinea pigs with cow's milk. These experiments have sufficed to show the presence of such bacilli in the milk, but they do not tell us whether the bacilli have come from the udder or not. The mere detection of the germs of tuberculosis in cow's milk does not prove that these have proceeded from the cow's udder. They may have come originally from the saliva or excrement of the cow, and have fallen in during the act of milking. By the method just described, however, it might perhaps be found possible to obtain more valuable results, and even to make test cultures of diseased milk.

II. PURE COUNTRY MILK.

In the previous section we have shown that healthy cows give milk which, examined by the usual methods, is absolutely free from germs. It appears, therefore, that the normal milk of the cow is milk not only undecomposed, but free from the germs of decomposition. "Pure" milk may possibly be defined as milk which has not been watered or adulterated. It is sometimes so defined. But normal milk cannot be defined in this way, and in the considerations which will follow the high standard of normal milk must be kept in view. Clearly, also, the

cow is not the source of the hosts of bacteria regularly observed in Boston milk. We therefore turned to the examination of milk as it is found upon the tables of well-to-do country families; that is, to the study of unquestionably "pure" country milk. Such milk is generally comparatively fresh, and is often served only a few hours after the milking. Nevertheless, our results show that it is generally rich in bacteria, though the numbers are much smaller than in ordinary city milk. We then made the following experiment: milk was drawn from a clean and well-kept Holstein cow in an unusually decent stable. The normal milk of this cow had already been repeatedly proved to be sterile. Milk drawn by hand from this cow with great care into sterilized bottles, and planted quickly, yielded, as an average of several trials, 530 bacteria per cubic centimeter. When, however, the milkman used the ordinary milk-pail of flaring form, seated himself with more or less disturbance of the bedding, and vigorously shook the udder over the pail during the usual process of milking, we found that the numbers were very much higher, namely, an average of 30,500 per cubic centimeter, at the end of the milking. When such milk is found upon the tables of country families a few hours later, it naturally shows still more bacteria, doubtless because those with which it was seeded have had time to multiply. The average of fifteen such samples from the tables of families in Jamaica Plain, Cambridge, and Auburndale, was 69,143 per cubic centimeter. In these cases, moreover, the conditions of the cows and of the stables were exceptionally good, while the milkmen were much more than ordinarily clean and careful.

It follows from these results that there are two principal sources of the bacteria in milk; namely, contamination during the act of milking, and the natural multiplication of the bacteria thus introduced during the interval between milking and the consumption of the milk. The result of these investigations was to show that even under the most favorable conditions cow's milk as ordinarily drawn becomes, almost necessarily, infested with hosts of putrefactive bacteria at the very outset. Under worse conditions, with unclean stables and dirty milkmen, to say nothing of half-cleaned pails and cans, it is easy to understand why milk swarms with bacteria; and, if we allow time also, the wonder is, not that it contains so many germs, but rather that it is still potable at all.

When we reflect upon the indescribably filthy condition of many cow stables; upon the fact that the cow's udders and flanks are not

infrequently covered with flaking excrement; upon the quality of the men employed to do the milking, etc., etc., it becomes a simple matter to understand how this rich animal fluid — sterile at the start, but drawn by unclean hands into half-cleaned pails, and meantime sprinkled from above by the dust of the stable, by hairs, dandruff, dirt, and particles of excrement from the skin and udder of the cow vigorously shaken by the milker or brushed by his hat — becomes infested with organisms. That these multiply swiftly and enormously in the warm and rich fluid, well aërated by the act of milking, is also a natural consequence of favorable conditions.

III. BOSTON MILK.

If, now, we turn to the condition of city milk as exemplified by that delivered in Boston (and we have no reason to suppose that Boston is worse in this respect than other large cities), we find, as might be anticipated, a marked increase in the numbers of bacteria over those found in country milk. The average number of bacteria found in fifty-seven samples of Boston milk in the spring of 1890 was 2,355,500 per cubic centimeter (roughly equal to a cubic one third inch or to one small thimbleful). The samples came from different parts of the city. Thus:

Charlestown,	8 samples averaged	4,222,500	per cc.
Jamaica Plain,	10 " "	3,259,600	"
Roxbury,	17 " "	1,874,300	"
South Boston,	9 " "	2,778,000	"
North End,	6 " "	708,100	"
Back Bay,	7 " "	1,189,800	"

The samples were obtained directly from the milk wagons and were planted at once. We are indebted to Dr. Charles Harrington, Milk Inspector of Boston, for much courteous assistance in procuring these wagon samples.

From groceries we obtained sixteen more samples, in which the average number of bacteria was 4,577,000 per cubic centimeter. The groceries usually have older milk than that found in the wagons, which readily accounts for the higher numbers of bacteria.

Ten samples collected from well-to-do families upon the Back Bay showed an average of 1,438,000 per cubic centimeter. The lowest number found in Boston milk supplied in the ordinary way was 30,600 per cubic centimeter. With special care, however, milk can be, and

now is in a few cases, regularly delivered in Boston with much greater freshness and purity than these figures indicate.

It will be seen at a glance that the milk supply of Boston is very rich in bacteria. Much of the milk arrives from very distant points by rail, and is therefore necessarily more or less stale. Forty-four samples of this so-called "railroad" milk from one dealer showed that the milk, even upon its arrival, contains on the average more than 500,000 bacteria per cubic centimeter. The extremes in this case were 5,664,000 and 2,200. In another set of ten samples the milk, on its arrival in Boston, averaged 371,000 per cubic centimeter.

Our experiments have been repeated in 1891, and the results prove that the figures for 1890 are not exceptional. There are some indications that, as might perhaps be expected, watered milk contains fewer bacteria than unadulterated milk. "Skimmed" milk, on the other hand, probably on account of its staleness, is sometimes very rich in organisms, though this is not always the case.

IV. INTERPRETATION OF THE RESULTS.

We have now shown that the normal milk of the cow is free from bacteria. We have also found that the milk supply of Boston is exceedingly rich in bacteria. We have further discovered that these are principally introduced during the operation of milking in unclean stables, and that they afterwards multiply enormously in the milk, in which they effect important changes of decomposition. Two principal conditions thus coöperate to cause the extraordinary abundance of bacteria in Boston milk; namely, *uncleanness* and *staleness.* The former furnishes the seeds of decomposition; the latter the time for their development. Their coöperation produces a fluid widely different from normal cow's milk.

It remains to inquire what is the probable effect of this condition of the milk supply upon the health of the community. Here we are very much in the dark. There can be no question that most of the milk is consumed when cooked, and even when raw, without the least apparent injury and with great apparent benefit. But it is probably also true that the use of stale and partially-decomposed milk, charged with living bacteria, has its effects upon invalids and children, and particularly upon infants, and that these effects are not always beneficial. It is possible that one explanation of the great mortality of children under five years of age, and especially of bottle-fed children, is to be sought for in this direction.

It will not do to argue, because healthy adults drink polluted and stale milk without obvious injury and with evident benefit, that invalids and infants may safely do the same. In order to learn the consequences of a battle, the investigator must examine not merely the survivors; he must consider also the fallen. If it be admitted that city infants, children, and invalids require normal cow's milk, it cannot be denied that they are now rarely, if ever, fortunate enough to get it. Many parents who are fastidious to the last degree concerning their own wine or table linen, provide for their children cow's milk which is both stale and filthy. It is safe to say that if our soups or our drinking water were drawn from cows, in remote and obscure stables, by ordinary milkmen, and shipped, manipulated, and delivered as our milk supply is, we should appreciate and resent the pollution. At present, however, so far as mere pollution is concerned, it is probably true that milk is actually improved by the addition of pure water. The public inspection of milk in America is usually directed mainly to the prevention of fraud; rarely, if ever, to the question of pollution, or, except in a very general way, to the protection of the public health.

"One point deserves in the future much greater attention. This is the *pollution* of milk. No food material can be so much polluted as milk. If any one will compute how much cow's excrement an infant swallows, and how much excrement an adult consumes in drinking the sewage-polluted water of the Isar, he will find that the latter is by far the better off."

Dr. Sohlet, the author of the foregoing paragraph, has lately urged that milk be examined not only in respect to its solids and fats, but also as to its contents in filth, after a method employed by Professor Renk, of Halle. The latter found that the public milk supply of Halle was polluted by very considerable amounts of filth, which settled to the bottom of the vessel containing the milk, and upon microscopical examination proved to be largely cow's excrement. The average of thirty tests showed fifteen milligrams per liter, or fifteen parts per million, of such filth deposited by the milk of Halle. In Leipzig milk, Renk obtained 3.8; in Berlin, 10.3; in Munich, 9.0 milligrams of similar filth deposited per liter. He found the bacteria in the Halle milk supply varying from 6,000,000 to 30,000,000 per cubic centimeter, a result agreeing well with its unclean condition. It may be remarked in passing that the sewage of American cities seldom contains, on the average, more than 1,000,000 bacteria per cubic centimeter.

V. REMEDIAL MEASURES.

It is clear that the improvement of the conditions which now exist must consist in greater cleanliness in milk production and less delay in its delivery. So far as pollution is concerned, it is possible to make great improvements by requiring cleaner stables; those which exist at present being often (from the standpoint of mere decency) simply abominable. The cows might also be kept much cleaner, and, if necessary, washed above and upon the udder with warm water before milking; or, at least, cleaned and groomed as horses have long been. The milkers might be compelled to have clean hands, and taught how to avoid pollution of the milk. That the milk-pail might be improved, and even kept partly covered, has been demonstrated by at least one invention now actually in use.

If the milk could thus be kept decently clean and nearly free from bacteria — the seeds of decomposition — and then quickly chilled to a low temperature, its decay would be very slow, and its "keeping qualities" might be greatly increased. It could afterwards be transported, over even long distances, without damage, provided only that a very low temperature were constantly maintained. Under existing methods, however, quick delivery is essential, since the milk is richly seeded with filth at the very outset, and only imperfectly chilled during its journey to the consumer. To permit of all these improvements it is possible that a higher price might have to be paid for clean and fresh milk, as is now done for fresh-laid eggs.

Thus far but little has been said of the dangerous possibilities of milk as a vehicle for the specific germs of disease. Stress has here been laid upon its staleness and uncleanness, and there can be no question that the dweller in Northern cities suffers from a milk supply affected with these conditions. The visitor to some Southern or tropical cities may witness a very different state of things. In Naples, for example, cows are driven through the streets, and the buyer of milk may have the satisfaction of seeing it drawn at his very door from a clean cow, to which the stable is unknown, while he himself witnesses the operation. The curious anomaly actually appears to exist that the milk supply of Naples is fresher, purer, and cleaner than that of Boston or New York. In Naples, in spite of urban life, the primitive method of milk supply still prevails.

But we are exposed not only to unclean, but also to infected milk, that is, to milk not merely polluted with filth, but also infected with

the germs of specific diseases, such as tuberculosis and typhoid fever. Numerous epidemics of the latter have been traced to infected milk, and it is now believed that the former is often conveyed in this most favorable medium. Whether scarlet fever and diphtheria are often carried in milk or not is still a question under discussion, but there is reason to believe that they may be so carried.

In view of all these facts we may well ask ourselves whether in the place of private care and watchfulness we have provided any efficient or adequate inspection of the stables from which the milk supply comes, or any even approximately adequate supervision of it after its arrival. There is at present, so far as I am aware, no systematic inspection whatsoever of the farms, stables, or herds from which public milk supplies come. After milk has arrived in the city, inspectors protect us more or less effectively from its adulteration with water. The dwellers in cities take such milk as comes, asking no questions. They have surrendered their primitive safeguards, and have placed none in their stead. By the present system of inspection we are protected from fraud, but not from filth or disease. Public milk supplies may not legally be watered, but they may be stale, or polluted, or infected.

JUL 2 7 1942